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ORIGINAL ARTICLES.

I.—On Certain Affinities between the Devonian Rocks of South Devon and the Metamorphic Schists.

By A. R. Hunt, M.A.

Part I.

(PLATES VI. and VII.)

In submitting the following paper to the readers of the Geological Magazine, I am conscious that some apology is needed for entering upon so difficult a subject. My position is as follows:—In 1879 the late Mr. E. B. Tawney joined me in the investigation of a series of detached blocks trawled from time to time by the Brixham fishermen. The character of certain of these stones having suggested to Mr. Tawney the idea that the schists of South Devon might possibly be of pre-Cambrian age,[1] he visited the district in 1880, with the expectation of being the first to advance that hypothesis. The evidence of the schists themselves failed to satisfy Mr. Tawney on the point in question, and his premature death prevented his attacking the problem on a subsequent occasion, as he had hoped to be able to do.

On Mr. Tawney's death in December, 1882, Prof. Bonney was good enough to take his place in supplying me with microscopic analyses of the Channel blocks. In the following Easter, a week spent among the metamorphic rocks of South Devon resulted in a paper published in the Quarterly Journal of the Geological Society, in November of the same year, in which Prof. Bonney expressed the opinion that the South Devon schists might be safely regarded "as Archæan, and as a prolongation of the *massif* so distinctly indicated in the Lizard, to which also belonged the gneiss of the Eddystone," Q. J. G. S., vol. xl. p. 23.

It may be observed that the original investigation of the Channel blocks was undertaken for the purpose of elucidating, if possible, the Start and Bolt problem, and that my first paper was lent to Mr. Pengelly in MS. for use in preparing his own paper, "The Metamorphosis of the Rocks extending from Hope Cove to Start Bay." The two were published in sequence at the Ilfracombe meeting of the Devonshire Association in 1879.

In 1879 Mr. Pengelly maintained the Devonian age of the metamorphic schists. During 1880–82 Mr. Tawney reserved his opinion,

[1] Trans. Devon Assoc. vol. xxi. p. 468.

having failed in his search for evidence of their pre-Cambrian age. In 1883 Prof. Bonney, without much hesitation, proclaimed the Archæan age of these troublesome rocks.

Under these circumstances there seemed nothing for me to do but to continue collecting facts, and to await the issue of events.

In 1887, 1888, and 1889, Mr. A. Somervail published three papers in the Transactions of the Devonshire Association, in which he suggested that the chlorite schists might be the representatives of the Devonian diabases which appear on the coast line of Start Bay, and in the neighbourhood of Dartmouth.

Early in 1891, Mr. W. A. E. Ussher was good enough to allow me to accompany him on two occasions when mapping the Devonian rocks between Dartmouth and Slapton Sands. Being much impressed by the many obvious analogies between these rocks and the metamorphic rocks further south, I determined to examine and compare as many varieties of the two sets of rocks as I could obtain.

My self-appointed task, then, has been to endeavour to ascertain what affinities, if any, can be detected between the metamorphic rocks of South Devon and the slates grits and volcanic rocks which lie to the northward of them; the green rocks being compared with the volcanics, the mica-schists with the slates, and the quartz-schists with the fine grits or sandstones.

At first sight the quartz-schists and grits seemed the least promising of the different rocks selected for comparison; but on Mr. Harker finding detrital tourmaline in one of the Devonian grits, and the same mineral being subsequently detected by myself in a quartz-schist from near Start Point, these siliceous rocks took a foremost place in the investigation.

The Quartz-Schists and Grits.

A geologist examining the classical area of Devonian rocks which forms the northern shore of Torbay between Hope's Nose and Hesketh Crescent, will not fail to notice the frequent interbedding of slates and grits (the latter often in bands too thin to be dignified by the name of sandstone), and that both rocks are often more or less micaceous.

A slice of a brown sandstone from the quarry on the path east of Kilmorie is seen in the microscope to be composed chiefly of fine grains of quartz, of which the larger average about $\frac{1}{200}$ inch in diameter, some of them being splinters with angles absolutely unaffected by attrition or solution; among these may be noticed an occasional flake of white mica, a few scattered fragments of tourmaline, and two or three grains of triclinic felspar.

Another specimen from the same quarry is a fine hard lead-coloured sandstone bedded in well-marked laminæ, which determine its fracture under the hammer; the surfaces of these planes of fracture being highly micaceous.

The bands of grit between slates are sometimes crossed by quartz-veins which do not invade the adjacent slates.

In the cliffs at the north-east end of Slapton Sands we meet

with slaty rocks with interbedded grit bands, in which latter mica is occasionally very conspicuous. The rocks here are less generally micaceous than those between Hope's Nose and Meadfoot in Torbay, but a reddish grit-band sliced for the microscope proved almost identical, except in colour, with the brown Kilmorie sandstone already mentioned; even to a few grains of triclinic felspar.

The cliffs at Torcross are passed unexamined, as there are some seven volcanic bands of more or less importance in them, and the sedimentary rocks in consequence liable to intermixture with volcanic ejectamenta.

Just south of the village of Beesands some interesting grits occur, about the spot where Sir Henry de la Beche placed the boundary-line of the metamorphic rocks. Again we meet with fine sandstones and interbedded grits and slates, the sandstones and grits occasionally containing tourmaline and mica, like the rocks at the north-east end of Slapton Sands, and those in Torbay, already described.

At a point on the coast, south of Start Farm and west of Start Lighthouse, among schists much crushed and altered, there occur occasional bands of quartz-schist, which in general appearance, in their constituents of quartz-grains, mica, and tourmaline, as well as in their intercalation between beds of a more slaty nature, recall to mind, both macroscopically and microscopically, the micaceous and tourmaline-bearing grit-bands of Torbay, Slapton Sands, and Beesands.

The undoubted Devonian sandstones may be traced into the undoubted metamorphic quartz-schists by four independent lines of inquiry, viz. by way of iron ores, tourmaline, mica, and quartz.

Iron Ores.

A slice of a grey micaceous sandstone (No. 5) from south of Beesands contains much brassy-looking pyrites crystallizing in cubes, rectangular prisms, and derivative forms.[1] The pyrites is occasionally seen to pass into red hæmatite. With strong oblique sunlight on the slide the characteristic colours of these minerals are well seen.

A slide (No. 22) from a grit-band between slates on the north shore of Southpool Creek, not far from the metamorphic boundary, contains a minute rectangular prism of pyrites, and flakes of red hæmatite.

In Hope Cove, south of Hope Headland, and therefore within the metamorphic boundary, a siliceous band occurs which, while showing no trace of its original quartz-grains, retains its pyrites in cubes and rectangular prisms.[2] The majority of these crystals display the brassy colour of pyrites, and occasionally characteristic striæ are seen. In one group of twinned crystals, a crystal is composed partly of yellow pyrites and partly of bright red hæmatite—whether in this case the latter is a pseudomorph or only a surface incrustation is uncertain.

In a slide from one of the slightly altered bands of quartz-schist south of Start Farm (No. 6) a cube of red hæmatite occurs which is

[1] Plate VI. Fig. 3.

[2] Plate VI. Fig. 4.

apparently a pseudomorph after pyrites, as the latter mineral has not been noticed in either of my nine slides of these quartz-schists, whereas red iron oxides are abundant.

Black granules and streaks, apparently magnetite, occur in all the above four slides, but as they in no instance exhibit the characteristic octahedral crystallization of that mineral, their evidence is only of minor importance. It is well worthy of notice that in three of these four slides, two being from the metamorphic district, and one from outside the boundary, we have pyrites intimately associated with hæmatite, either in the same crystal, or as a pseudomorph; and further, that on either side of the boundary we find pyrites crystallized in the somewhat uncommon form of rectangular prisms.

Tourmaline.

Tourmaline has been noticed in ten slides from the following six Devonian localities, viz. quarry near Kilmorie, Torbay; north-east end of Slapton Sands; cliffs south of Beesands; grit band in Southpool Creek; near East Charleton; and Aveton Giffard: and in eight slides from two bands of quartz-schist on the coast south of Start Farm, in the metamorphic area. In every case the larger grains are clearly of detrital origin, the only exception being in one or two instances in the quartz-schist, where secondary tourmaline has crystallized on the original grains, and these secondary crystals have been swept to a distance by differential movements in the rock.[1]

No alteration by pressure or solution has been observed in the tourmaline grains in rocks north of Beesands, the first indication noticed being south of that village.

In a reddish grit containing much hæmatite (No. 59), a well-characterized light-brown crystal of tourmaline is broken sharply in two, and slightly dislocated. Clear quartz fills the intervening gap without in the least dissolving the edges of the tourmaline crystal.[2]

A grain of tourmaline in another slide from the same neighbourhood (No. 5) is slightly, albeit distinctly, affected by solution, with indications of the re-crystallization of secondary tourmaline.

In a slice (No. 16) of one of the quartz-schist bands south of Start Farm, two crystals of tourmaline may be especially noticed: one, a longitudinal section, with characteristic transverse cracks; the other a basal section. In each case there is a considerable growth of secondary tourmaline on the original crystals; and in each case also, by a slight differential movement of the rock, fragments of this secondary tourmaline have been detached and moved to a small distance from the parent crystal.

We have thus close together in the same field of view detrital tourmaline and induced tourmaline.[3] The origin of the latter would be obscure, were not all the steps of the process so clearly set forth. Tourmaline occurs in many forms and colours; but the tourmalines of Beesands and the Start coast are identical.

[1] Plate VI. Fig. 2. [2] Plate VI. Fig. 1. [3] Plate VI. Fig. 2.

Quartz-grains.

The quartz-grains in the sandstones north of Beesands do not call for particular notice. Just south of Beesands we have seen quartz filling the interstices of a broken tourmaline, and in another case a grain of tourmaline slightly affected. Professor Bonney's description of a rock from the same locality seems exactly to cover the case, viz. : "The rock has evidently been much compressed, and some slight amount of mineral change has taken place" (Q.J.G.S. vol. xl. p. 18). An occasional quartz-grain contains liquid inclusions and bubbles, but without any distinguishing features.

A slide of a Devonian grit-band from Southpool Creek (No. 22) contains more than one quartz-grain of interest. One grain showing considerable solution at the edges abounds in hair-like inclusions together with an occasional fluid inclusion with bubble. Another grain is crowded with fluid inclusions, some of which contain very active bubbles, while others take the form of negative crystals.

In a slide (No. 8) cut from the specimen of quartz-schist from Start Farm, given me by Mr. A. Somervail, we find a quartz-grain with hair-like inclusions; and in another slide from the same stone (No. 10) we have a grain with bubbles in negative crystals. These two grains resemble the two in the Southpool Creek slide already referred to.

However, the most interesting feature in the quartz-schist lies in the fact that the rock has not been submitted to pressure and heat sufficient to obliterate entirely the original tourmalines and quartz-grains, and that a comparison with the sandstones and grit-bands of Beesands and Southpool Creek is still possible. In the case of the quartz-schists at the Bolt Head, further west, tourmaline is absent, and the obliteration of original structures in the quartz is complete; thus a comparison with the Devonian sandstones on this particular point is not possible.

Mica.

On first collecting the Devonian micaceous sandstones for comparison with the Start schists, the possibility that the mica in either case might be detrital had not been entertained. On Mr. Harker pronouncing the Slapton Sands mica detrital,[1] and that in the Start quartz-schists possibly so,[2] the origin of the Devonian and metamorphic micas became a problem of importance.

If the metamorphic rocks were micaceous before their alteration, the presence of much of their mica might be accounted for with much less dynamical and chemical metamorphosis than is at present demanded for them.

In the case of the sandstones and quartz-schists, it is easy to find Devonian sandstones much more highly charged with mica, than the Start quartz-schist under consideration, in which, as Mr. Harker remarks, the mineral is only sparingly present, *e.g.* the sandstones and grit bands of Kilmorie, Meadfoot, Slapton Sands and Beesands,

[1] Appendix, Slide No. 3. [2] Appendix, Slide No. 8.

to go no further a-field than the coast-line. Both the sandstones and quartz-schists contain mica varying between the minutest filmy flake, hardly seen in the microscope, and flakes sufficiently apparent to the naked eye. Given a small amount of solution in the silica of the quartz-grains, so that the mica flakes might be free to partially rearrange themselves, and there seems to be no reason why the Start quartz-schist under consideration should not have been derived directly from such a rock as the Kilmorie sandstone, without any development of newly-formed mica.

So far as I understand it, a theory held by some is that the Devonian slates and sandstones when micaceous are with very few exceptions "Phyllites," in which the mica is considered to be an induced product; and that the mica-schists are rocks in which the mica has also been induced but to an incomparably greater extent, so that the rocks differ in kind. It seems, however, possible, that the converse of this may be true, and that with some exceptions the mica in both the Devonian and metamorphic rocks is, or was, originally, detrital, the deposition of micaceous sediment being much more general in the southern area than further north. Irregular deposition is occasionally exemplified in the Torbay rocks, in which worm-tracks in slates are filled with fine micaceous silt.

In the foregoing pages the iron ores, tourmaline, micas, and quartz-grains of the Devonian sandstones have been connected with the iron ores, tourmaline, micas, and quartz-grains of the metamorphic quartz-schists. It cannot be doubted that were the rocks further examined by a competent mineralogist, other points of resemblance would be noticed, for minute grains of felspar, unless well defined, and the rarer minerals, are beyond the power of my microscope and knowledge to distinguish.

One noticeable feature which the grits and quartz-schists possess in common is the abundance of laminæ of iron oxides: at Hope's Nose, black; at Kilmorie, brown; at Slapton Sands, red; at Beesands, black and red; at the Start, black and red. The colours being probably due to the brown, red, and black, oxides of iron. The following remark of M. Daubree is worth noticing, although the black oxides of the Start schists do not take the form of crystallized magnetite:—

"Dans certaines localités des Ardennes, les cristaux de fer oxydulé, qui imprègnent les ardoises se sont logés suivant les longrains, et font ainsi ressortir des joints rudimentaires qui, ailleurs, ne sont pas reconnaissables à la vue."—Geol. Experimentale, vol. i. p. 336.

At the Start we find the quartz-schist traversed by numerous little cracks cemented by opaque iron-ores.

The Mica-Schists and Slates.

The mica-schists of the metamorphic district, as distinguished from the schists in which quartz is the most prominent mineral, have not been examined to any extent.

One specimen of a red schist, which I thought might compare with the red Devonian slates, is described by Mr. Harker as a rock

which "may have been originally a shale or slate with some gritty bands."[1] Mr. Harker could not have more precisely described the cliffs at the north-east end of Slapton Sands, with whose shales or slates I desired to connect the red mica-schist from the Start, had he been intimately acquainted with the locality instead of being a perfect stranger thereto.

End of Part I.

EXPLANATION OF PLATES VI. AND VII.

Plate VI.

Fig. 1.—Crystal of tourmaline in fine Devonian Sandstone, four chains south of Beesands. (59) Magnified about 37 diameters.

Fig. 2.—Crystal of tourmaline in quartz-schist, Start Farm. Formation of secondary tourmaline with partial dislocation of same. (16) Magnified about 37 diameters.

Fig. 3.—Rectangular iron pyrites in fine Devonian Sandstone, north of Tinsey Head. (5) Magnified about 18 diameters.

Fig. 4.—Rectangular iron pyrites in siliceous band, South of Hope Headland. (52) Magnified about 18 diameters.

Plate VII.

Fig. 1.—Quartz-schist, Start Farm. Remnants of original quartz grains. (Compare "Grain of Quartz-sand in the Mica-schist of Arroquhar." Presidential Address of Dr. Sorby, F.R.S., Proc. Geol. Soc. 1880, p. 86, Fig. 9.) Magnified about 12 diameters.

Fig. 2. Fine Devonian Sandstone, north of Tinsey Head, with schist-like alineation of iron ores and greenish mica. Magnified about 12 diameters.

I am much indebted to my friend Mr. W. M. Baynes, of Torquay, for valuable assistance in the preparation of my photographs for publication.

(*To be continued.*)

II.—Permian in Devonshire.

By W. A. E. Ussher, F.G.S., etc.

(By permission of the Director-General of the Geological Survey.)

In the course of a visit to the Hunsruck district between Treves and Bingen in the spring of 1890, in company with Professor Gosselet, Mons. C. Barrois, Herr von Reinach and Herr Grebe, I was struck by the resemblance displayed by the Permian quartz porphyry of the valley of the Nahe, between Birkenfeld and Bingen, to fragments contained in the breccias of Teignmouth. In discussing the South Devon section with the two gentlemen last named, on that occasion, they both gave it as their opinion that the lower beds of the Devon section might prove to be representatives of the Rothliegende.

In the pressure of other geological work, I had no time to carry the matter further, until May, 1891, when Herr von Reinach spent a week with me in examining the South Devon coast section and the eruptive rocks in the vicinity of Exeter and Crediton. He then expressed the opinion that certain parts of the lower New Red rocks of Devon corresponded very closely with members of the German Permian, but that this correspondence was not borne out in con-

[1] Appendix, Slide 24.

secutive horizons. In the Teignmouth breccias with numerous fragments of claystone porphyry, quartz porphyry, etc., he detected a strong resemblance to the Rothliegende (Upper Söterner) of the Nahe, above the volcanic horizon (Grenz schichten). In the Volcanic rocks of the Exeter and Crediton areas he recognized the equivalents of the Grenz schichten (Middle Söterner). Herr von Reinach also pointed out to me the similarity presented by some of the thick even-bedded red and whitish sandstones intercalated with marls to the east of Exmouth, to the Upper Bunter Sandstones of the Moselle, recalling to my mind sections I had seen near Treves and in the Eifel.

On the 10th of last February Herr von Reinach wrote me as follows:—"Professor Bücking had the kindness to determine the melaphyres and tuffs I brought with me from the environs of Exeter. As I do not intend to publish these facts, I authorize you, also in the name of Professor Bücking, to make use of this communication. You know that we have at the Nahe—(1) Melaphyres in the Older Rothliegende which are only intrusive; (2) Melaphyre Covers (Melaphyr decken) which we have only in the Söterner (Grenz schichten) of the Rothliegende. The Grenz Melaphyre has been divided by Lossen into Upper, Middle, and Lower, which types are generally the same throughout the Nahe district." Prof. Bücking's identifications are as follows:—

Spencecombe, Knowle Hill (Crediton Valley) { Mesokeratophyr or Orthophyr. / Biotit Melaphyr with Olivine.

Posbury, near Crediton / Pocombe, near Exeter } Melaphyr with Olivine.

Dunchideock, between Exeter and Chudleigh.—Melaphyr with Olivine and Bronzite.
Yeoton, near Crediton.—Tuff.

Herr von Reinach adds: 'The Dunchideock Melaphyr is identical with our type of the uppermost Melaphyr roof zone (Dach zone) of the environs of Kreutznach on the Nahe.'"

Although the detailed mapping of the eruptive rocks associated with the New Red of Devon was completed about fourteen years ago, I have published nothing specially on that subject, certainly not from lack of materials. In 1876 Mr. Rutley visited all the chief localities with me, and determined the rock for the Geological Survey; these determinations have been lately confirmed by Dr. Hatch, and they agree with the determinations of Prof. Bücking; of this Herr von Reinach was not aware, but the fact rather enhances than lessens the importance of the communication he has so kindly authorized me to impart.

If we assume the correlation of the New Red volcanic rocks with the Grenz-schichten as established, the probable correlations with the Nahe section might be as follows:—

Dawlish breccias	Upper Rothliegende.
Teignmouth breccias	Lower Rothliegende. (Söterner beds with volcanic rocks).
Basalts (local)	
Watcombe and Petitor conglomerates...	

It must, however, be borne in mind that no correlations framed on a partial, or even intimate acquaintance with the South Devon coast

section only, can be regarded as conclusive. The local clusters of volcanic patches from Exeter northwards may not be on the same horizon. As regards the Bunter, if we assume the Straight Point (east of Exmouth) Sandstones to be Upper Bunter, this probable correlation favours the Middle Trias age assigned to the Lower Marls in 1878. No evidence has been adduced to invalidate my classification of the Keuper from the Pebble beds of Budleigh Salterton upwards. I may say that Mr. Geo. Spencer Perceval, in three letters to the "Western Morning News" in 1877 or 1878 (I forget which year), disputed my classification of the Budleigh Salterton Pebble bed in the Keuper, maintaining that it represented the Bunter Pebble beds; this naturally deprives Prof. Hull's recent communication to the Geological Society of the charm of novelty for me.

My old friend Mr. P. O. Hutchinson, of Sidmouth, in 1878, found remains of an Equisetum-like plant in greenish shaly sandstone in the lower part of the Keuper Marls near Sidmouth; this occurrence suggests a possible comparison with the "Schilf (Reed) sandstone" of the Middle Keuper of the Eifel. But an Equisetum (*E. Mougeoti*) occurs in the Upper Bunter sandstone of the Eifel as Herr von Reinach informs me, so that no reliance can be placed on this suggestion. I take this opportunity of recording the recent discovery (a few months ago) of quartz porphyry giving place upwards and outwards to a rock resembling a mica andesite and identical with specimens from one of the volcanic patches associated with the New Red of the Crediton Valley. This discovery was made at the eastern termination of the Thurlstone New Red Outlier. In connexion with it I quote the following passage from my paper[1] before referred to:—"The breccias frequently contain igneous fragments distinctly referable to the destruction of such igneous patches as those of Washfield, Kellerton, etc. The outflow of these lavas seems generally to have accompanied or heralded the earliest deposition of Triassic sediments in the districts in which they occur; *nor does it appear impossible that the eruption of quartz porphyries may have been in some way connected with their appearance.*"

The late Professor Ramsay inspected the New Red Rocks in 1880, and failed to find any sufficient proof for the correlation of the lower breccias with the Permian of the Midlands. There is not a shred of proof that the two areas were connected until the Keuper, and probably at a late stage in that period. The Midland sections owe their importance as a basis for correlation entirely to the merits of the assumed correctness of their classification with reference to the German types. For much information on the characters of the German types I am indebted to my friend Herr von Reinach, whose acquaintance with them is most intimate. Until I have the opportunity of availing myself of his kind offer to show me the rocks on the ground, or until new facts are brought to light in Devon, I fail to see that the literature of the New Red Rocks of Devon and West

[1] Quart. Journ. Geol. Soc. for Aug. 1878, p. 462.

Somerset,[1] supplemented by the present communication, will be benefited by the addition of more speculative correlations.

My friend Dr. Hatch has kindly sent me the following notes on specimens of the New Red Volcanic rocks of South Devon.

Notes on Sliced Specimens of the Exeter "Traps," in the Collection of the Geological Survey at Jermyn Street.

Olivine-basalt or *melaphyre*, composed of lath-shaped striped felspars and scattered grains of magnetite, with calcite, replacing augite, and ferruginous pseudomorphs after olivine.

Localities: Raddon Court (No. 951), Pocombe (No. 953), between Chiphele and Budlake (No. 958), and Quarry near Crabtree, Kellerton (No. 945).

Porphyrite (*Andesite*): A felted aggregate of lath-shaped and microlitic striped felspars, with occasional porphyritic crystals of plagioclase. *Localities*: Western Town, Ide (943), Quarry N.E. of Knowle, N.E. of Holcombe-Burnell (946), and Knowle Quarry, W. of Dunchideock (949). The last two, with rounded (corroded) grains of quartz.

Mica-porphyrite (*Mica-andesite* or *trachyte*). — Finely vesicular rocks containing brown mica, imbedded in a minutely microlitic ground-mass, which is generally rather obscured by the presence of dusty magnetite and secondary calcite.

Localities: Kellerton Park (Nos. 944, 947 and 950). The nature of the felspar in these rocks is indeterminable, but specimens from Copplestone (Nos. 957 and 959) consist of a holocrystalline aggregate of broad lath-shaped crystals of orthoclase, flakes of brown mica and magnetite.

III.—Did the Mammoth Live Before, During, or After the Deposition of the Drift.

By Henry H. Howorth, M.P., F.G.S., etc.

In recent papers which I have printed in the Geological Magazine I have tried to show that some of the greatest mountain chains in the world are of very recent origin; and that this accounts for their showing no traces, or very slight ones, of the action of ice on a wide-spread scale.

I have, in fact, ventured to lay down the conclusion that the presence or absence of such traces of ice-action is a test of whether these mountains existed at the so-called Glacial period or not.

If my view be right, it follows that very large masses of land were thrown up suddenly or very rapidly in post-Pliocene times; and, if this was so, it is very probable that there was a great subsidence of land in other parts corresponding to this upheaval. I believe that this can be proved, and that it involves some important

[1] Geol. Mag. for April, 1875; Quart. Journ. Geol. Soc. Aug. 1864; February, 1870; Nov. 1876; Aug. 1878; Transactions of Devonshire Association, 1877, 1878, 1881; and the Proceedings of the Somersetshire Archæological and Natural History Society for 1889, vol. xxxv.

and interesting considerations worth discussing. Before we can deal effectively with this problem, however, we must settle another one equally important and equally interesting, to which I propose to devote a few pages, namely, the precise geological horizon where we are to put the Mammoth and his companions, including Palæolithic man.

It is at once a reproach to geology, and a good proof of the very great difficulty there is in making our way among the latest geological records, that there should be any doubt upon such a question. The fact is nevertheless so, and has given rise to a sharp polemic elsewhere in quite recent years. I am bound to say that I myself have changed my views on the subject in view of the evidence, and have to recant some phrases I once printed in this MAGAZINE.

I wish to speak with some precision in the matter, and not to be misunderstood. The point I would discuss is not whether the Mammoth lived before, during, or after the so-called Glacial period, but whether the beds in which his remains are found, when undisturbed, underlie or overlie the Drift, or are intercalated with it. The two questions are not, as is often assumed, the same, and it is to the latter alone that I wish my criticism to apply.

I must also define the kind of evidence which I alone think conclusive. I altogether distrust any evidence on the point except that of superposition. Evidence based upon inferences of different kinds I have always mistrusted in deciding this critical question, but I would go further. We must remember that the Till or Boulder-clay contains extraneous bodies of different kinds, which are often far-travelled, and sometimes not so. Among these bodies there may be tree-trunks or molar teeth of Elephants, which may be as true boulders as those of granite and gneiss and like them derived from elsewhere. Whatever theory we adopt in regard to the Till, we must concede that it picked up far-travelled débris in its march, and mixed it in many cases with the débris of the underlying beds, sometimes with Chalk, sometimes with Lias, sometimes with sandstone and mixed this débris with the fossils from these different beds. It is further clear that ice would take up and convert Mammoth teeth into boulders just as readily as Ammonites and Belemnites, and that the former would be no more contemporary with the Till nor evidence of an interglacial climate than the latter are. Let us now turn to the evidence.

The earliest discovery of Mammoth remains in Scotland took place in the year 1817. "When," says Sir A. Geikie, "the Till covering the sandstone at the quarry of Greenhill in the parish of Kilmaurs, in Ayrshire, had been partially removed, there were found at a depth of 17½ feet from the surface, two Elephant's tusks. . . . The matrix in which they were found was a clay, which around the bones changed from its usual light brown colour into a dark brown, with a most offensive smell when turned over. The tusks lay in a horizontal position with several bones near them. Dr. Scouler visited the quarry about 1840, and reports that seven

more tusks had subsequently been found there (Trans. Geol. Soc. Glasgow, vol. i. pt. 2, pp. 68–69). Some antlers of the Reindeer were also found along with an Elephant's tusk at this quarry (*id.* p. 71).

These remains were found in a clay containing, inter alia, *Astarte compressa* and *Leda pygmæa*, with eight genera and nine species of *Foraminifera*, and five genera and ten species of *Ostracoda*, and also a number of seeds of plants and fragments of beetles. The earlier writers described this clay as Boulder-clay. Dr. Bryce, in 1864, opened some pits at the place, and found that the beds in which the remains occurred *underlie* the Till, and he put them on the same horizon as the Forest Bed. Mr. Craig and Mr. Young, in 1869, after a close examination of the district and the position of the beds in regard to the Boulder-clay, satisfied themselves that the Mammoth and shell-bed were pre-Glacial (see Trans. Geol. Soc. Glasgow, vol. iii. p. 310). The officers of the Geological Survey in their commentary on Sheet 22 describe the bed as inter-Glacial; but after sifting the evidence again, and writing in 1887, Mr. Craig says: "I see no cause to change the conclusions arrived at in our joint paper of 1869." Dr. Bryce's surmise that the fossiliferous bed may be the equivalent in age of the Cromer Forest Bed is, in my opinion, not far from being correct. Elsewhere he says he places the beds at the very base of the Glacial deposits (Trans. Geol. Soc. Glasgow, vol. viii. pp. 213–223).

According to Mr. J. Geikie, these remains "occurred in a peaty layer between two thin beds of sand and gravel, *overlaid by Till or Boulder-clay*, and resting directly on the sandstone rock of the quarry" (Great Ice Age" p. 605).

A horn of a Reindeer was also found in the basin of the Endrick, in the parish of Kilmarnock, about four miles from Loch Lomond. "The section in which it occurred," says Sir A. Geikie, "consisted first of the vegetable mould, then of a stiff clay 12 feet thick, containing a large quantity of stones, underneath which was a bed of blue clay about 7 feet thick, *at the lower part of which, close upon the* underlying sandstone, the antler was found, and near it a number of marine shells. The deposit was estimated to be about 100 to 103 feet above the sea-level" (Trans. Geol. Soc. Glasgow, vol. i. pt. 2, pp. 70, 71; see also Dr. Smith, Edinb. New Phil. Journ. n.s. vol. vi. p. 105).

The next locality where Mammoth remains occurred in Scotland was during the excavation of the line of the Union Canal, between Edinburgh and Falkirk. This was in 1820. A large mass of Boulder-clay having been undermined fell into the cutting, and among the earth was found a tusk measuring 39 inches in length. It had lain about 15 or 20 feet from the surface. Mr. Bald, to whom the discovery was reported, examined the place. He says it was found from 15 to 20 feet from the surface. He did not actually take it out of the clay, and only judges that it must have come out of it from its excellent state of preservation (Wernerian Society, vol. iv. p. 60). It has subsequently been described as having been imbedded in the heart of the stiff clay. If it had been so, it can

only be treated as a boulder, since it was quite detached; this is suggested by Mr. Bald, and it in no way evidences an old land surface; but it seems to me there is no warrant for describing it as having come out of "the heart of the stiff clay."

A skull of *Bos primigenius* was found in 1868, near Croft Head in Renfrewshire, imbedded some few feet deep in a soft clay or mud, interlaminated with lines and beds of sand, and occasional layers of fine gravel. In some of the layers of clay there was a little vegetable matter in a state of decay. These beds were *overlain* by Till full of scratched stones (Geikie, GEOL. MAG. Vol. V. p. 393, etc.).

In a paper on this deposit, in Vol. VII. of the same MAGAZINE, Mr. Geikie says that it subsequently yielded remains of the Horse and Irish Deer. He reaffirms the opinion that the overlying clay in this instance is the true Boulder-clay, that it is *in situ*, and in no sense due to a landslip.

On the 6th of March, 1879, there was exhibited before the Geological Society of Glasgow a well-preserved molar of a Mammoth found four years before in sinking a pit on Mainhill Farm, near Baillieston, east of Glasgow. It was found in a bed of purely laminated sandy clay, at a depth of 33 feet below the present land surface, the sand bed being from 40 to 45 feet thick, and resting directly upon the rock-head or Coal-measures without any intervening Till or other superficial strata. In cutting the line of railway leading to the pit, the sand bed was seen to be overlapped by a thick bed of stiff dark-coloured Boulder-clay full of large travelled stones, which thinned away as the pit was approached, and the sand bed rose to the surface. Mr. Young remarked that here we had another instance of the occurrence of the Mammoth in Scotland during pre-Glacial times, and he went on to remark that in all the cases which had occurred in Scotland, the Mammoth remains he says "had either been derived from pre-Glacial beds below the Till or from the Till itself. Dr. Geikie has never been able satisfactorily to show that the Mammoth-bearing inter-Glacial beds, in any of the places where they have been found, rested on an older Boulder-clay, although he says that at other spots, such as Kilmaurs, intercalated beds are found in the same district between the two Tills; but unfortunately for his contention, these beds have never as yet yielded any Mammoth remains, nor any of the other organisms found associated with them. When traces of the Mammoth have been got in Boulder-clay, they may in all probability have been derived from the denudation of pre-Glacial beds in the same district" (Proc. Geol. Soc. Glasgow, 1878–9, p. 291).

In January, 1882, there was exhibited before the Glasgow Geol. Soc. a fragment of a tusk of the Mammoth which had been found in sinking a pit on the farm of Drummuir, Dreghorn. This was found in a bed of sand *underlying* 76 feet of Boulder-clay (Trans. Geol. Soc. Glasgow, vol. viii. pt. ii. p. 213, etc.).

In a paper by Messrs. Craig and Young, in the third volume of the Trans. of the Geol. Soc. of Glasgow, they say, *inter alia*, that if we look at the recorded instances of the occurrence of the Mammoth

and Reindeer in Scotland, we shall find that in the majority of cases they either have been found in beds below the Boulder-clay or in that deposit itself. Those found in the Boulder-clay may have been derived from the denudation of pre-Glacial beds. In the one or two instances where the Mammoth has occurred in beds more recent than the Till, they may still have been worked out of it. Bearing on the pre-Glacial age of the Reindeer in Scotland, they refer to a portion of the right antler of a Reindeer found in the Boulder-clay at Raes Gill, Carluke, Lanarkshire. "The specimen bears evident marks of transportation, its burr, browtyne, and other extremities being worn and rounded, and the whole surface has a smooth, polished, ice-scratched appearance, exactly like that we meet with amongst the ice-worn stones of the Till. It was found in Till several feet thick" (*op. cit.* pp. 310–320).

Let us now turn to England. No remains of Pleistocene mammals have occurred so far as I know in the counties of Northumberland, Durham, Cumberland, or Westmoreland.

Of the few cases of the discovery of Mammoth remains in Lancashire and Cheshire, the only one, according to my friend Prof. Dawkins, in which the relation of the deposit to the Boulder-clay is clear, was the famous case of the discovery made by Mr. Bloxsom in March, 1878, in digging a shaft for the new Victoria Salt Co. near Northwich, of a fragment of a molar. It occurred at a depth of 65 feet in a bed of sand overlying the red Keuper marls, and *overlaid* by 37 feet of brown Boulder-clay (Q.J.G.S. Feb. 1879, pp. 140–141).

If we turn to Yorkshire, the evidence is more abundant and more conclusive.

In a Memoir on the Moors, Mountains, and Sea-Coast of Yorkshire, published by J. Phillips in 1853, he urged that the lowest Hessle gravels, which rest upon Chalk, and are covered by Boulder-clay, as well as the contents of Kirkdale cave, are pre-Glacial. Writing in 1868, he says of this view about the Hessle gravels: "I am still disposed to favour this opinion; in the first place, there is no proof that these beds are marine, but a strong presumption to the contrary, from the considerable abundance of land Mammalia found in them, especially *Elephas primigenius* and Horse; and secondly, beds of this order composed of chalk and flint fragments, not only are not known to occur in the midst of the Boulder-clay, but can hardly be imagined to exist there; and, thirdly, the Boulder-clay rests on them without conformity (Q.J.G.S. vol. xxiv. p. 255).

Phillips continued during half a century to be the advocate of the Mammoth beds being older than the Drifts, and in the last edition (1875) of his Geology of Yorkshire, in describing the ossiferous marls of Bulbecks, near Market Weighton, shows that they lie directly on the red marls; and in regard to their relative age, he says: "It appears to be proved, both by comparison with the analogous deposits at Hessle and Bridlington, and by the superposition of the ordinary diluvium in the south-eastern part of the Vale of York, that the latest of these inundations (*i.e.* that which

laid down the Mammoth beds) was anterior to the movement of waters which brought many Cambrian rocks through the pass of Stainmoor and dispersed them over the hills and valleys and antediluvial lake deposits of Yorkshire" (*op. cit.* pp. 12–19).

Speaking of the Hessle beds containing Mammalian remains, he says: "As they are now covered up by a great thickness of clay and pebbles derived from a far greater distance (*i.e.* by the Drift), we count them the spoils of pre-Glacial land" (*id.* pp. 57, 58).

Teeth of Mammoths sometimes occur in the Boulder-clay in Yorkshire as they do elsewhere, but are clearly derivative and boulders. Phillips has remarked how in certain places only the teeth occur, and no other parts of the skeleton (*id.* p. 74); when bones occur under these circumstances, they are always scattered and generally rolled (*id.* p. 170).

In regard to the deposits in Kirkdale cave, he points out their analogy with those occurring at Bulbecks, "where glacial drift overlies the bones," adding "that Kirkdale cavern was occupied in the pre-Glacial condition of the land which is now Yorkshire was my earliest opinion, and seems still to be the most probable inference in the present state of knowledge" (*id.* 169–171).

This opinion about Kirkdale cavern is interesting, for the evidence is fast accumulating to show that the cavern deposits at all events are older than the distribution of the Till. I should like to refer to another northern cavern, where, although no Mammalian remains occurred, there seems to have been a very decided invasion of Drift.

Speaking of the cavern at Stainton-in-Furness, Mr. Cameron says: "The floor of a gallery resembles the bed of a dry mountain torrent, being strictly strewn with water-worn pebbles and boulders. Soft yellow clay occurs, frequently also gravel; while again in other places there is a pavement of hard dry clay split up by cracks into octagonal-shaped masses. . . . In this gallery are also Silurian boulders, often cemented together in huge masses. A few of these boulders are of a larger size than to have allowed of their entrance through the as yet only known inlet to the place. Ireleth, about $4\frac{1}{2}$ miles off, is the nearest place where this rock is *in situ*, and boulders and fragments of rock are often met with, thrown against each other in the direst confusion as if impelled along by a very strong current and suddenly stopped" (Geol. Mag. Vol. VIII. pp. 312, 313).

We will now turn to the famous Victoria Cavern, where a considerable polemic arose in regard to the interpretation of the facts. It must be remembered that this discussion was before the more recent discoveries of Dr. Hicks, etc., in North Wales. Prof. Dawkins, who took the view in the paper that the remains in this cave were post-Glacial, says in the discussion that he could not say whether the Victoria Cave was pre-Glacial or Glacial, nor even define its relation to the Glacial period. The age of the clays was, he said, a matter of opinion (Q.J.G.S. vol. xxxiii. p. 612).

Other explorers of the cavern were much more emphatic in their view. Mr. Tiddeman, who drew up the report to the British Asso-

ciation, distinctly refers the deposit to a pre-Glacial age, and speaks of the Craven savage as having lived before the Great Ice-sheet (Rep. Brit. Assoc. 1875, p. 173).

In a paper on the Cave by the same author, he tells us how a bed of tenacious clay with scratched Silurian and other boulders was found underneath all the talus at the mouth of the cave, resting on the edges of the beds containing the older mammals, and dipping outwards at an angle of 40°. Mr. Tiddeman explains this as the remnant of a moraine (lateral or *profonde*) which dammed up the mouth of the cave and prevented anything but water charged with fine sediment from entering it during the Glacial period. Perhaps, he adds, one of the strongest pieces of evidence, that the older cave animals lived in this district only at a time previous to the great ice-sheet, is that, so far as we know, the remains of none of them (except of *Cervus elephas*) have ever been found in any of the post-Glacial deposits of this district. Though so common in the river gravels in the Midland and Southern Counties, they are never found except in caves until we get much further south or east. Leeds is, I belive, the nearest locality where they occur. This would seem to imply that their remains were wiped off the area by the great ice-sheet, . . . and only left in the shelter of caves to which it could have no direct access" (Geol. Mag. Vol. X. p. 15).

Writing in "Nature," the same geologist says, "A human bone or fibula was certainly found beneath glacial clay in the Victoria Cave" (Nature, vol. xiv. p. 505). A dispute arose afterwards as to whether the fibula was human or not, but this does not affect the issue we raise. Mr. Tiddeman again says, "In the Victoria Cave the surroundings are such that nothing but an ice-sheet could have sealed up with glacial clay the remains discovered by the Committee. . . . The origin of the boulders, their position, the ice-scratches on the rocks hard by, all point to the time of greatest glaciation, when the whole district was covered in with ice and snow of great thickness. And the agent which closed the cavern and concealed the animals within it must have been the same which swept the country clean of their remains all around further than the eye can reach" (*ib.* 506.)

In the discussion on Prof. Dawkins' paper, Sir A. Ramsay said he thought the evidence for the existence of Man in the Victoria Cave before the Glacial period was stronger than that against it. Prof. Prestwich thought the deposits in the Victoria Cave were pre-Glacial (Q.J.G.S. vol. xxxiii. p. 612).

The next case to which I shall refer is one in which we have not to deal with Mammalian remains, but with the Southern freshwater shell the *Cyrena fluminalis*, which marks the Pleistocene horizon in other places.

In 1861 Professor Prestwich read a paper before the Geological Society of London on the occurrence of *Cyrena fluminalis* over beds of Boulder-clay near Hull. He says there was previously no evidence of direct superposition to show the age of the shell. I quite agree with him that for this reason the instance in question is important if maintainable. Is it so? In the first place in the pit where the

Cyrena occurred in thousands, and of which Mr. Prestwich gives a section (Q.J.G.S. vol. xvii. p. 450), *no Boulder-clay at all occurred.* Secondly, in another section at Paull Cliff, near Hull, where the *Cyrena* also occurred in fewer numbers, there *was an underlying clay*, but Prestwich admits that inasmuch as it contained neither boulders nor fossils, he could not feel certain about its being the Boulder-clay" (*id.* 452). Mr. Prestwich then had some experimental borings made, but they did not succeed in piercing the gravel, and therefore, in his own words, "failed to obtain the exact proof" (*id.* 453). Hence the evidence as tested by this locality utterly fails. Now it is curious that while Mr. Prestwich failed to find Boulder-clay in a definite position in regard to the gravels, Messrs. Wood and Rome did, and they say: "The gravel of Kilscar Hill, the subject of the notice of Mr. Prestwich, is (now that the ballast pit has been more extensively worked) shown most distinctly *to be overlain by the Boulder-clay, no less than* 15 *feet of it being so exposed in one part*; and they add that there are no means of ascertaining at present on what it rests (Q.J.G.S. vol. xxiv. p. 153).

Let us now turn to the evidence of the Mammals. This also seems to be very conclusive. I will first refer to the well-known memoir by Mr. C. Reid on the Geology of Holderness. In this memoir, chaps. v. and vi. are headed "Inter-Glacial Beds." I do not know why, for I confess I can find no evidence of their *inter-Glacial* character. That is, however, another issue.

Mr. Reid objects, on the evidence of the fossils, to Professor Phillips treating the Hessle gravels as pre-Glacial, an argument in which I do not quite follow him. He admits that at Bridlington they rest directly on the Chalk (*op. cit.* p. 48), that is, have no Boulder-clay or true Drift below them. There some Mammalian remains have occurred in a buried cliff, which has since been very carefully examined by Mr. Lamplugh, to whom I shall refer presently. His conclusion that this bed underlies the basement bed of the Glacial series is confirmed by an observation of Mr. Reid, who says, that a similar bed of chalk-gravel, in borings at Bridlington Harbour, rests on the Chalk, and is clearly beneath the Basement clay (*id.* p. 49).

Turning to the Hessle Mammaliferous gravels on the Humber, Mr. Reid admits that they also unmistakeably rest directly on the Chalk, and are covered and overlapped by Boulder-clay. Yet he goes on to argue that "we have nothing to fix the age of the Hessle gravel by, and that there is no positive evidence whether another underlying Boulder-clay has been denuded or not." If so, why call the bed inter-Glacial? And he goes on to say, "Prof. Phillips's reference of the Hessle gravels to a pre-Glacial period *may turn out*, with fuller evidence, to be well founded." I should have said that it was conclusively proved, there being against it no stratigraphical facts, but merely an *à priori* prejudice based on some theory about the fossils.

A similar bed, but without fossils, at South Ferriby Cliff, on the south of the Humber, also rests on the Chalk, and is overlain by Boulder-clay.

In regard to the marine gravels in which Professor Prestwich found the *Cyrena fluminalis*, and which have yielded many Mammalian remains, Mr. Reid admits that at one spot, at Kelsey Hill, a face of about 12 feet of weathered Boulder-clay, with small stones, can be seen *overlying* the gravel (*id.* p. 54).

Mr. G. W. Lamplugh, in his paper on the Drifts of Flamborough Head recently published, is most clear on the subject. He describes the buried cliff at Sewerby as having been formed by marine action prior to the deposition of any of the Glacial beds, and as having afterwards been buried and obliterated by the accumulation of materials banked against it (Q.J.G.S. vol. xlvii. p. 394). In a later paper, where he enters into greater detail, he puts the Sewerby gravels, which have yielded so many Pleistocene Mammalian remains, distinctly *under* the whole Glacial series, and notably under the so-called basement bed. He comes to precisely the same conclusion in regard to the marine shell bed at Speeton (*op. cit.* pp. 410–412). In the discussion on this paper Mr. E. T. Newton describes the remains of mammals from Sewerby as just such an assemblage as might be expected in an undoubted Pleistocene deposit.

It seems to me, therefore, that the Yorkshire evidence, wherever we can test it, agrees with that of Scotland and of Cheshire in compelling the conclusion that the horizon containing remains of the Mammoth and its companions is distinctly below the Drift beds.

(*To be continued.*)

IV.—The Reported Occurrence of *Ammonites jurensis* in the Northampton Sands.

By S. S. Buckman, F.G.S.

In the Geological Magazine (Dec. III. Vol. VIII. No. 329, p. 493, Nov. 1891), Mr. E. T. Newton has a "Note on the Occurrence of *Am. jurensis* in the Ironstone of the Northampton Sands in the Neighbourhood of Northampton." This note had especial interest for me, from my having contested Mr. Beeby Thompson's view of the *Jurensis*-zone being represented by the clayey beds beneath the Sands; and, in fact, it seemed to support my argument. Further reading, however, obliged me to confess that I could not claim this support; for Mr. Newton says (p. 494), "that the specimens of *Am. jurensis* agree exactly with Dr. Wright's plate 79 (Lias Ammonites)."

Now, in 1887, I pointed out that this form of Wright's was very different to Zieten's *jurensis*, and that it occurred in the *Opalinum*-zone;[1] and, in 1888, having occasion to refer to it in some sections, I named the form afresh *Lytoceras Wrighti*.[2] Further, the same form is figured by Branco, under the name *Lytoceras dilucidum* on the authority "Dumortier (non Oppel)."[3]

The form called *Lytoceras Wrighti* differs materially from Zieten's

[1] Inferior Oolite, etc., Proc. Cotteswold Club, vol. ix pt. ii. p. 134, footnote, 1887.
[2] Monogr. Lias Amm. pt. ii. p. 44, footnote, Pal. Soc. 1888.
[3] Untere Dogger, Abh. geol. spez. karte Elsass-Lothringen, Bd. II. pl. i. fig. 8.

original *Am. jurensis* in the amount of involution, the extent of compression, the very different-shaped umbilicus,[1] and the broader whorls. Even if the term 'species' be interpreted in a very liberal sense, to include both forms as *jurensis*, the differences remain the same. Wright's fossil is not the typical *jurensis*; and, moreover, it is a form characteristic of the *Opalinum*-zone. Whether it be regarded as a different species, or only a mutation of *jurensis*, it is advantageous to mark its differences by a name—*Lytoceras Wrighti*.

The evidence of the *Jurensis*-zone in the Northampton Sands is thus narrowed down to *Am. insignis*. The typical form is highly characteristic of the *Jurensis*-zone; but there are several mutations and allied species which are characteristic of the *Opalinum*-zone. For instance, the "*Insignis* var.," which Newton says agrees with Wright's pl. 75, figs. 1–3, is quite an aberrant form of *Hammatoceras* (*Insignis*-group). It differs especially from *insignis* in ornamentation, and in having ventral furrows. As it is not named, to my knowledge, I think it might appropriately be called *Hammatoceras Newtoni*. It occurs in Dorset quite high up in the *Opalinum*-zone. Its nearest allied form is *Am. Alleoni*, Dumortier, of the same zone, which is less coarsely ornate, less sulcate, and more involute.

So far the bulk of Mr. Newton's evidence seems to indicate the Northampton Sands being the *Opalinum*-zone, a conclusion I expressed in Monogr. Amm. p. 53, 1888. The occurrence of *Am. Murchisonæ* in the *Opalinum*-zone has been noted by myself and several other authors; but the real head-quarters of *Murchisonæ* are on a distinctly higher level than the head-quarters of *Opalinum*.

As regards the *Jurensis*-zone in Northamptonshire, my position is simply agnostic. I know no Ammonite-evidence,—barring, perhaps, Mr. Newton's "*insignis*"—to show the existence of any of the four levels—*Dumortieria*-, *Dispansum*-, *Striatulum*-, *Variabilis*-beds—of this zone, which are easily recognized by an abundant Ammonite-fauna for about 80 miles from Haresfield to the Dorset coast; but the species of some or all may ultimately be found to occur in the Northampton Sand, and I doubt not in the same successional order.

Of the *Opalinum*-zone there is good evidence for the existence of the upper level *Opalinum*-beds; of the lower level, *Moorei*-beds, *Lytoceras Wrighti* appears to be the only evidence. This is not sufficient—see sections Monogr. Amm. p. 45, et seq.

A summary of the species allied to or similar to *A. jurensis* may be useful in this connexion. As their genealogy, synonymy, etc., will be given more fully in a future paper, only the briefest notice will be made.

1. *Am. jurensis*, Zieten. Oval whorls, very little involute. Very rare. Burton Bradstock, Yeovil Sands.

2. *Am. perlævis*, Denckmann (usually called *jurensis*). It differs only in being more compressed ventrally, and having a more pronounced inner margin. Scarce. *Dispansum*-beds, Cotteswolds.

3. *Am. phyllicinctus*, Quenstedt. Like the last, but more evolute.

[1] The umbilicus of the typical *Wrighti* is basin-shaped, that of the typical *jurensis* wider and graduated.

Only a fragment. Bottom Yeovil Sands (*Dispansum*-beds), Bradford Abbas.

4. *Lytoceras sigaloen*,[1] n. sp. S. Buckman (*Ammonites jurensis*, d'Orbigny (non Zieten), Pal. franç. pl. 100). It has been thought that this is the species Oppel intended to call *Amm. dilucidus*; but Oppel's reference is to Quenstedt's *Amm. fimbriatus opalinus*, which is that author's name for *cornucopiæ*, d'Orbigny (non Young). Further, Oppel says, "that *Amm. cornucopiæ* and *Eudesianus* are the nearest allies to *dilucidus*." It is reasonable to suppose that had he meant to call d'Orbigny's *jurensis* by the name *dilucidus*, he would have said "that Zieten's *jurensis* was the form nearest to *dilucidus*." *Lyt. sigaloen* is like *perlævis*, but more compressed and more involute. A magnificent specimen, 16 inches (406 mm.) in diameter, is in my collection from Yeovil Sands, Haselbury, Somerset.

5. *Lytoceras Wrighti*, S. Buckman. Monogr. page 44, footnote, 2, Pal. Soc. 1888. (*Lyt. jurense*, Wright (non Zieten), Lias Amm. pl. 79.) Like *sigaloen*, but very involute. Basin-shaped umbilicus. *Opalinum*-zone, Dorset, Somerset, Gloucestershire, etc.

The above species, so far as their suture-lines and inner whorls are known, show clearly enough genetic relationship with *Amm. Germanii*, d'Orb., *torulosus*, Zeit., and *hircinus* (Schloth.), Quenstedt. They lack entirely the extraordinary lengthy lobes—especially the superior lateral—of the *Fimbriatus*-group.

6. *Ammonites linulatus*, Quenstedt (Am. Schwäb. pl. 48, fig. 2), is another form which may belong to the same group. It differs from *jurensis* in having a fan-shaped aperture with a high inner margin. Whorls depressed. Foreign only. *Jurense*-zone.

Three other species have resemblance, but are not exactly allied to *jurensis*.

7. *Am. amplus*, Oppel. A quick-coiled, very inflated form. Not known in this country. Relationship uncertain. *Murchisonæ*-zone.

8. *Lytoceras confusum*, S. Buckman (*jurensis* auctorum). A slowly-coiled form with triangular aperture, and marked shoulder. *Concavum*-zone, Dorset. The inner whorls and young specimens sometimes show periodic ribs indicating relationship with *ophioneum*, Benecke, and *rasile*, Vacek.

9. *Amm. trapeza*, Quenstedt. A slowly-coiled, very evolute form, with subtriangular whorls, and more rounded-off shoulder. Very rare. Sherborne-zone with *Amm. fissilobatus* (*Sowerbyi*-zone of German authors).

V.—The Revised Astronomical Theory of Glaciation.

By G. W. Bulman, M.A., B.Sc.;
Corbridge-on-Tyne.

In "The Cause of an Ice Age" Sir R. Ball claims to have removed from the Astronomical Theory of Glaciation the greatest stumbling block in the way of its general acceptance, and to have placed it on a firm and unassailable foundation. And this stumbling block he considers to have been Sir J. Herschel's

[1] σιγαλόεις, smooth.

erroneous statement, that, of the total heat received by a hemisphere in a year, one-half is received during the summer, and the other half during winter.

Sir R. Ball has worked out the true distribution of the yearly heat between the seasons, and arrived at figures about which he claims there can be no question. His result is, that of the total yearly heat received by a hemisphere, 37 per cent. is received during winter, and the remaining 63 per cent. during summer. Granting these figures, it is maintained that glaciation follows as a necessary consequence, whenever the difference in the lengths of summer and winter is sufficiently great.

This difference in length between the two seasons formed the starting point of Dr. Croll's Theory. It was supposed by him that the winter supply of heat being spread over a greater number of days, the temperature would be so reduced, and the snowfall so increased thereby, that finally—with the aid of ocean currents, etc. —glaciation would result.

And in showing that only 37, instead of 50, per cent. of the yearly heat is received during winter, Sir R. Ball *appears* to have removed a difficulty; 37 per cent. of the yearly heat would certainly not maintain so high a temperature as 50 per cent. during the lengthened glacial winter. But a little consideration will show that the removal of the difficulty is only apparent.

Our *present* average winter temperature forms the *starting point* for the calculation of what it would be when the winter was at its longest, and this calculation can be made *quite independently of the figures* 37 and 50. The *result* will be *the same* whether we assume 37 or 50 per cent. of the yearly heat to be received during winter, *for these figures do not necessarily enter into the calculation at all.* They did not, as far as I am aware, enter into that of Dr. Croll, who reasoned correctly, that our present winter supply of heat, when spread over 199 days instead of 179, would produce an average temperature lower in proportion to the lengthening of the season.

The *difficulty* in connexion with the supposition of half the yearly heat supply coming in winter is rather that of understanding how a winter climate could be produced at all; for our winter being *shorter, if* it received the *same amount of heat,* would be actually *hotter* than summer.

But granted that 50 per cent. of the annual heat supply permits a winter climate during a portion of the year at present, the lengthening of the season might be supposed to reduce the temperature as easily as on the supposition that the percentage was 37 per cent.

Sir R. Ball considers that Dr. Croll was unacquainted with the unequal distribution of heat between the seasons, but he has not shown that any of the latter's reasonings or calculations depend on the assumption of its equal distribution.

The theory set forth in "The Cause of an Ice Age" is regarded as a revised form of Dr. Croll's, but the two theories differ in certain essential particulars. For, in the first place, while the Revised

Theory attributes glaciation solely to the lengthening of one season at the expense of the other—in other words to purely astronomical causes, viz. eccentricity and the precession of the equinoxes—Dr. Croll attributed it to physical agencies brought into operation by these astronomical causes. Among these physical agencies by far the most important is the Gulf Stream. But according to the Revised Theory the deflection of this great ocean current is not required to produce glaciation, and Sir R. Ball supposes it to have flowed much as it does now during the Glacial period. And there are two considerations which seem to show that the Gulf Stream *did* modify our glacial climate, and which were, therefore, difficulties on Dr. Croll's view. The first of these is, that palæontological evidence seems to show, that the western shores of our island were warmer than the eastern—as they are to-day. And the second is, that in North America glaciation extended fully 10° further south than it did here.

Again, while, according to Dr. Croll, the astronomical conditions might occur without leading to glaciation through secondary physical causes, the Revised Theory *requires a group of glacial* periods whenever the eccentricity is sufficiently great. Dr. Croll has indeed shown how the astronomical causes which under certain conditions —of distribution of land and water, etc.—would produce a group of glacial, separated by genial, periods, might, under other conditions, lead to a group of genial periods separated by others a little less genial. And thus—especially in his later writings—he seems to have allowed considerable force to Sir Chas. Lyell's views on the cause of glaciation, though only as adjuncts to the Astronomical Theory.

It is probably in the judicious combination of the two theories that the true solution of the glacial problem will have to be sought.

While, then, the essence of Dr. Croll's Theory consisted in the *continued increase* in the cold, due initially to a lengthened winter, by physical agencies thereby brought into action, the Revised Theory attributes glaciation entirely to the reduction in temperature produced by spreading the winter heat supply over a greater number of days.

The question, then, of how far our present winter temperature would be reduced if its heat supply were spread over 199 days—the longest possible winter—is the critical point in the Revised Theory.

If it can be shown that the consequent reduction of its winter temperature would place our climate on a level with those of countries now glaciated; and that the summer supply of heat—as great as at present though concentrated into fewer days—would be unable to melt the ice and snow, then glaciation follows as a necessary result of the lengthening of the winter. But this has not yet been done.

Sir R. Ball (Appendix pp. 177, 178) gives data from which the reduction in our average winter temperature during the winter of 199 days can be found.

At present he calculates we receive a mean daily average of 0·75

of a heat unit during winter, and when that season is as its longest 0·68, which gives a difference of 0·07. Now each *tenth* of a unit corresponds to a rise or fall of 30°, and hence 0·07 corresponds to $\frac{7}{10}$ of 30° or 21°.

Our present average winter temperature may be taken as 42°, and hence during the long winter it would be 21°. This would give us a climate milder than that of Greenland between the 10° and 0° January isotherms, since its *mean* winter temperature is probably not more than 6° above this.

The climate represented by this winter average of 21° may perhaps be fairly compared to that of America between the 20° and 10° January isotherms which include the southern part of the region of the Great Lakes. That such a reduction in temperature would produce glaciation is not then apparent.

Using a different method of calculation, however, I have arrived at a much lower temperature for the winter of 199 days.

Assuming, as Sir R. Ball has done, that the direct heat of the sun is expended in keeping the earth above —300°, and taking our mean January temperature of 36·9° for convenience of comparison with the January isotherms, we have the following calculation:

336·9°, the temperature maintained by the direct heat of the sun at present, $\times \frac{179}{199} = 303°$, the same when that heat is spread over 199 instead of 179 days.

This gives a mean Jauuary temperature of 3°, or a mean winter temperature of 9°, if we suppose this latter 6° in excess as at present.

This would probably give us the winter climate of some part of that strip of Greenland before alluded to, and lying between the S.E. coast and a line drawn from about Clavering Islands on the east to the point where the Arctic circle cuts the west Coast. This would indicate a climate like that of the above glaciated region; but is it a necessary consequence that such a winter would produce perpetual snow in our latitude?

We must remember that the heat received during summer would be the same in quantity as at present, though concentrated into fewer days; and the question to be decided is, would this heat be sufficient to melt the snow of the previous winter or not? It is not quite obvious that it would be, and the point seems one for careful consideration and calculation.

What, for example, would be the effect on the ice of Greenland if we could transfer our summer there for a few years without altering its winter? One is inclined to suggest that a gradual clearing of the country from *perpetual* snow and ice would take place. And a comparison of the part of Greenland under consideration with the neighbourhood of Lake Superior and Quebec which lies between the same January isotherms is instructive. For while the former is glaciated, the latter is not; nor are those portions of Asia south of 60° N. lat., lying between the same isotherms. And the reason of this is doubtless their much hotter summers. Hence, although our summer heat supply may be less than that of the above

regions, it is still sufficiently greater than that of Greenland to make it difficult to believe that a mean winter temperature of 9° would reduce our country to the present state of the latter.

We may, then, conclude that a mean winter temperature of 21°, or even of 9°, does not *necessarily* imply glacial conditions *like those of Greenland at present.*

From Sir R. Ball's data again, the average summer temperature during the most genial period possible, viz. when the summer is 199 days and the winter 166, can be calculated. The present mean daily heat of summer is 1·24, and during the summer of 199 days it would be 1·16. This gives a reduction of 0·08 of a heat unit, and since each tenth of a unit corresponds to a fall of 30°, we have $\frac{8}{10} \times 30° = 24°$ as the difference in average temperature between our summer and that of the most genial period. If we take the July isotherms as rough guides to the average summer temperature, this would give us a *Greenland summer* during the most genial period. Such a summer climate can scarcely be supposed to have tempted the plants and animals of the tropics or subtropical regions to wander north to our latitudes.

If, then, the burden of accounting for tropical, or subtropical, as well as glacial periods is to be laid on the Astronomical Theory of Glaciation, the revised form must be declared wanting.

But these reductions of winter and summer temperatures depend largely on the number —300, which has been used in the calculations, and its use requires some justification. If the *whole yearly heat supply* of a hemisphere had been reduced, and not *merely the daily average of winter*; or if the earth in the absence of the sun could *very rapidly* cool down to —300°; then the above calculations might be taken as approximately true. But the yearly heat supply is not appreciably altered. Moreover, the earth—or particular hemisphere under consideration—has a certain temperature due to the heat of the previous summer; and *this temperature must share with the winter heat supply the task of maintaining the hemisphere above* —300°. Suppose this temperature resulting from the heat of the previous summer to be 55°. If, in the absence of the sun, the earth could sink from this to —300° in a few days, then this factor might be neglected.

But supposing that instead of the above rapid rate of cooling it required, say, six months to cool down to —100°; then, instead of —300, we should have to use —100 in our calculations, and this would make important differences in our reasonings. I am not prepared to assert that —100 is the correct number, but it seems almost certain that —300 is too low, and that the earth would not sink to so low a temperature in six months. For the *rate* of cooling of a heated body diminishes rather rapidly with the diminution of its *excess* of temperature. Thus the rate of cooling for an excess of 80° C. is little more than a *tenth* of what it is for 240° C. Again, it has been shown that the rate of cooling diminishes with the density of the surrounding air up to a pressure of $\frac{1}{100000}$ of an atmosphere. But the earth's heat before it can radiate into stellar space must

pass through the exceedingly attenuated upper strata of the atmosphere. Again, if cooling down to —300° within six months were possible, we should expect to find some approach to this in the lowest recorded temperatures of the Arctic regions during the absence of the sun. It is true that in these regions the temperature is modified by warm winds and waters. Yet on still, clear nights, in places removed from the influence of ocean currents, these modifying causes will be reduced to a minimum. I have not myself come across any record of temperature so low as —80°, and even if we suppose it sometimes sinks lower, we have still no indication that —100° is too high for our calculation. And we have to remember, as a set off against the influence of winds and currents, and probable lower temperatures than any yet recorded, that the intense cold of the Arctic regions may be due to other causes besides radiation. The *evaporation* which takes place from the surface of ice and snow itself reduces the temperature already, it may be, far below freezing. Snow, again, falling on the open sea *melts* even in water already below freezing, and thereby adds to the cold.

And there is another consideration which seems to show that —100° or even higher is a more suitable point to reckon from than —300°. According to Sir R. Ball the present difference in the mean daily average of heat between summer and winter is 1·24—0·75, or 0·49. If $\frac{1}{10}$ of a unit means 30° difference in temperature, then ·49 would be equivalent to $30° \times 4{\cdot}9 = 147°$. And our present July temperature is 60°! The average difference between summer and winter is about 20°. This would seem to indicate that —50° is the most correct starting-point.

Taking —200° or —100° as starting-points, then, according to Sir R. Ball's calculation, the tenth of a heat unit will mean 20° or 10° instead of 30°, and the reduction in the average winter temperature would be $\frac{7}{10} \times 20° = 14°$, or $\frac{7}{10} \times 10° = 7°$. This would give 28·3°, or 35·3°, as the average winter temperature during the winter of 199 days.

With these figures, then, the Revised Astronomical Theory seems insufficient to account for glaciation. On the other hand, they permit to a warmer summer during a genial period.

It appears, then, that *a suitable temperature as a basis for calculation is the imperative need of the Theory, and that the use of* —300° *has not been justified.*

In his statements concerning the effects of varying eccentricity and the precession of the equinoxes on the length of the seasons, Sir R. Ball is not quite so clear as might be desired on a point of such primary importance to his Theory. Thus, on p. 95, we find the statement that, "*with the present eccentricity* of the earth's orbit, the great possible difference between summer and winter would amount to 33 *days*. I do not mean that the actual disparity between summer and winter at the present moment is so much as this; it only, in fact, amounts to seven days, because at present the line of equinoxes does not happen to be adjusted in the manner described."

But on page 97 we read: "*When all circumstances combine* to accentuate as much as possible the difference in the lengths of the seasons, one of them may be 199 *days long and the other* 166."

Again, on page 153, we find the statement that seven days is the greatest possible difference between the seasons under present eccentricity: "*So long as the eccentricity of the earth's orbit remains at its present value*, the difference between the lengths of the seasons will fluctuate between *the extreme values* of a winter seven days longer than a summer, and a summer seven days longer than a winter."

Yet, as we have seen, it is expressly stated, on page 95, that the present position of the line of equinoxes is *not* such as to produce the maximum difference possible for the present eccentricity. And on page 167 it is stated that the present seasonal difference is "*near its maximum* for the present eccentricity of the orbit."

The true law is stated on page 152 thus: "The differences between the lengths of the seasons is, as a mathematician would say, a function of two other independent quantities; it partly depends upon the eccentricity of the earth's orbit, and partly on the longitude of the perihelion, that is to say, on the position of the line of equinoxes with respect to the longer axis of the earth's orbit; if there were no eccentricity, there could be no difference in the lengths of the seasons, no matter where the line of equinoxes may lie. On the other hand, no matter what the eccentricity may be, there would be no difference in the lengths of the seasons if the line of equinoxes passed through perihelion." That is to say, for *each* value of the eccentricity the difference in the length of the seasons may vary from zero to a certain maximum value determined by the eccentricity. This maximum difference, Sir R. Ball states, is found by multiplying the eccentricity by 465. Taking ·0167922, the figures given by Sir J. Herschel as the present eccentricity, we obtain 7·8 days as the maximum difference. This maximum will occur when the line of the equinoxes is perpendicular to the major axis of the ellipse of the earth's orbit; and the seasons will be equal when it coincides with the major axis. And by what is called precession of the equinoxes this line describes complete circles round the sun as centre, and thus takes up successively every possible position in relation to the major and minor axes of the earth's orbit.

With the greatest possible eccentricity the difference between the seasons may attain to $0{\cdot}0745 \times 465$, that is, 33 days.

In the above quotations the italics are mine.

A slip appears to occur on p. 81 in reference to Fig. 2, p. 82. Instead of "the part AB is comparatively near the sun, while the other part XY, is as far as possible from the sun," should we not read, "the part AB is as near the sun as possible, while the other, XY, is comparatively far from the sun"?

And on p. 82, when we read, "it therefore sweeps across from X and [to?] Y in much less time than it takes to pass from A to B," the reverse is doubtless intended.

Sir R. Ball has brought forward certain facts in botanical distri-

bution as witnesses of the truth of the Revised Astronomical Theory. The facts, however, seem to me rather to testify against it. The argument is briefly thus:

It has long been known that certain temperate forms of plants in regions far removed from each other and *on opposite sides of the equator*, exhibit a remarkable similarity.

So great is the likeness that it is supposed that those in the south must have migrated from the north, or *vice versa*. They must, then, *have crossed the equator*, and that they did so is a *proof* that equatorial regions were at one time cooled down sufficiently to permit of the existence in them of these temperate forms of vegetation. And the Astronomical Theory *requires* that equatorial regions in the glaciated hemisphere should be thus cooled down. Thus the botanical facts seems to confirm the Astronomical Theory.

But, although the lengthening of one season will *reduce* its average temperature, that of the other will be *increased* by the corresponding shortening. And if the present heat of the tropics during summer in our northern hemisphere would be fatal to temperate plants, much more would it be so during glaciation when the summer supply of heat was concentrated into 166 days. A calculation of what the temperature of the glacial summer would be from Sir R. Ball's figures will perhaps make this more obvious. The mean daily heat during the glacial summer is given as 1·38, and at present as 1·24, which gives a difference of ·14. Now if ·1 of a heat unit corresponds to 30°, ·14 will correspond to 42.° Taking the present average temperature of our equatorial regions as 80°, this gives 122° as that of the glacial summer!

The botanical facts, then, seem to testify that if a glacial period helped the plants over the equator, glaciation was not brought about according to the Revised Astronomical Theory.

In seeking in the stratified rocks for evidence of the numerous glacial periods which must, according to the Astronomical Theory, have occurred in the past, Sir R. Ball's method of interpreting the record has at least the merit of novelty. Dr. Croll, and other geologists, when in search of such evidence have weighted themselves with the necessity of finding scratched and polished boulders, erratics, and other deposits analogous to those formed during the latest ice age. Sir R. Ball has rid himself of this impediment, and finds in the *ordinary alternation* of stratified beds *a record of alternate mild and glacial epochs*.

The scarcity of evidence of former Ice Ages, sufficient to satisfy the average geologist, is well known, and has hitherto formed one of the most—if not *the* most—serious stumbling blocks in the way of the Astronomical Theory of Glaciation. And the Revised Theory suffers more in this respect than did that of Dr. Croll. For according to the former glaciation follows as a necessary consequence, whenever the requisite astronomical conditions occur, viz. whenever by combination of high eccentricity and the approach of the line of equinoxes to a position perpendicular to the major axis of the earth's orbit, the difference in length between the two seasons becomes

sufficiently accentuated. Dr. Croll, on the other hand, has shown, as we have seen, that, according to his views, the astronomical conditions may occur *without* leading to glaciation. Hence, according to the Revised Theory, a larger number of glacial periods in the past is necessary, and hence a larger amount of evidence of such should be expected from the geological record.

The *imperfection of the record* doubtless accounts for much, yet it is difficult to believe that glacial periods could have occurred in the past with the frequency demanded by the Revised Astronomical Theory without leaving much more evidence of their occurrence than has yet been forthcoming.

In conclusion, I have not wished in the foregoing remarks to show that the Astronomical Theory is insufficient to account for glaciation. I have rather endeavoured to call attention to certain calculations which will have to be made, and certain facts which will have to be established before the Revised form can be generally received as a satisfactory explanation. Thus it will have to be shown, by careful calculation and comparison with existing climates, that the reduction of the average winter temperature by the lengthening of that season would be sufficient to produce in our latitudes a snowfall comparable to that of Greenland at the present day. And granting such a snowfall, it will have to be shown that all the heat of the succeeding summer—*the same in amount as now*—would be insufficient to melt it. And in order to calculate the reduction in temperature, due to the lengthening of the winter, a suitable datum line of temperature will have to be fixed upon, after a careful consideration of the numerous factors involved.

The supposed confirmation of the Astronomical Theory in the present distribution of temperate plants north and south of the equator, I have endeavoured to show is rather a contradiction, since the burning heat of the equatorial summer during glaciation would render the passage of such plants across the equator even more unlikely than it is to-day.

I have demurred again to the suggestion that Sir R. Ball's discovery removes any difficulty from the older Theory of Dr. Croll, and have drawn special attention to the essential differences between the two views. I have pointed out that the newer view more urgently demands evidence of former glacial periods in the geological record, and noted the easy way in which Sir R. Ball proposes to meet this demand.

REVIEWS.

I.—Geology of the Central Himalayas. By C. L. Griesbach, C.I.E., Superintendent, Geological Survey of India. Memoirs of the Geological Survey of India, Vol. XXIII. 1891, pp. 232, Pls. 27, Phototypes in the text 31, and 2 Maps.

BY the term 'Central Himalayas' the author includes that portion of the system of mountain ranges fringing the southern margin of the the Tibetan Highlands, in which the headwaters of the Ganges drainage are situated, and which extend to

the north-west as far as the Sutlej gorge. Within this area the Central Himalayas may be divided into a Northern range, in which is the water-shed of the region, and a Southern range which contains the highest peaks. This latter is mainly formed of crystalline rocks, chiefly gneisses and metamorphic schists, whilst the Northern range is almost entirely composed of a vast thickness of sedimentary strata ranging from the lowest Palæozoic to late Tertiary. This memoir mainly relates to the character of these sedimentary deposits, which are now elevated so as to form a high rim round the southern edge of the great high plateau of Húndés, which in its lowest portion is 12,000 to 16,000 feet above sea-level. The principal geological features of this region have already been made known in the works of General Strachey and Dr. F. Stoliczka; the present report, which is the result of nearly twelve years' investigation, very materially adds to our knowledge of its geological structure, and in some respects considerably modifies the descriptions of the earlier writers.

The region under consideration is indeed a wild one; a large part of it is above the line of perpetual snow, which in the Southern range is between 15,000 and 16,000 feet above the sea, and in the Northern somewhat higher. Nearly every valley above these elevations has its glacier, and some of these range from five to sixteen miles in length. A fact, interesting to glacial geologists, is noted by the author, viz. that during years of observation he failed to find striated and polished boulders in the moraines of these glaciers, whilst the bounding rock-walls of the glacier-valleys do not show the smooth and rounded surfaces, usually seen in other regions, and it is supposed that subaerial denudation and rapid weathering has obliterated the ice-markings. From the presence of morainic material at lower levels, the author concludes that the glaciers must formerly have extended much further than at present. In one respect the geologist has an advantage in these high mountain regions, for the rocks are not hidden by vegetation, and in the absence of snow the main stratigraphical features can be noted readily even from a distance.

The main mass of the Himalayas consists of an enormous thickness of crystalline rocks, in which two systems can be recognized; an older of granitic gneiss, which is now known to be metamorphic granite; and a younger series of micaceous schists, talcose rocks, phyllites and gneiss, to which the name of "Vaikrita" system is applied. Both the lower gneiss and Vaikrita systems are interpenetrated by veins and masses of eruptive granite, which also traverse the next overlying Haimanta rocks.

The lowest distinctly sedimentary rocks, which rest, without any clear line of division, on the Vaikrita schists and gneiss, consist at their base of thick beds of conglomerate and purple quartzites, succeeded by shales and phyllites, and limited above by a zone of bright red quartz shales, with an estimated thickness altogether of 4000 feet. This series, named the "Haimanta" system, is probably the equivalent of the Cambrian and older divisions, and may correspond with Strachey's azoic slates. The only organisms yet found in them are traces of Crinoid-stems, casts of bivalves and of *Bellerophon*.

Conformably succeeding the Haimanta rocks in the sections between the parallel ranges of the Central Himalayas, is a series of shaly quartzites alternating with coral limestones about 300 feet in thickness, which, on the evidence of the fossils, are referred to the Lower Silurian, and resting on these are quartzite beds about 1000 feet thick, which are placed as Upper Silurian, although the fossils in them have not yet been fully determined.

Next in ascending order are dark limestones, about 650 feet in thickness, containing Corals, Crinoids and Brachiopods, which are doubtfully placed as Devonian; following these are red crinoidal limestones considered to be Lower Carboniferous and white quartzites and dark limestones with *Productus*, belonging to the Upper Carboniferous. In Spiti, limestones containing *Athyris royssii*, *Productus*, etc., probably represent the highest beds of the Carboniferous in this region.

According to Mr. Griesbach's experience, there is a gradual passage between the different Palæozoic formations in the Himalayas, and from the phyllites and quartz shales of the Haimantas or Cambrian, to the Upper Carboniferous, there is a perfect and continuous sequence without the slightest unconformity. There are evidences of considerable physical changes at or near the close of the Carboniferous period, and the strata of this age are in places unconformably overlapped by shales with *Productus*, considered to be Permian. The interruption between the Carboniferous and Permian is followed by another long interval without a break, which continued until after the deposition of the Liassic limestones.

The Permian *Productus* shales, not over 250 feet in thickness, are followed by the lowest beds of the Triassic series containing *Otoceras*. This series in the Northern range of the Himalayas consists mainly of limestones, dolomites and shales, in all about 4000 feet in thickness. The resemblance between the Triassic rocks of this region and the Alpine facies of the same formation, long since recognized by Salter, Strachey and Suess, is confirmed by Griesbach, who divides this series into Lower, Middle, and Upper, corresponding respectively to the Bunter, Muschelkalk, and Keuper of Germany. The Rhætic series of the Himalayas consists also of limestones and dolomites, and resting on them are shelly limestones of Liassic age, having a thickness altogether of 2000 to 2500 feet. As in the Triassic, there is also in these Rhætic and Liassic rocks a very strong resemblance to the corresponding formations in the Alps, not only as regards the fossils, but in lithological character as well.

There seems to be an unconformity or interruption between the Lias limestones and the next succeeding Spiti shales, which contain Ammonites and other fossils of Middle and Upper Jurassic type.

Resting on the Jurassic Spiti shales there are sandstones and shales with Belemnites probably of Cretaceous age. Above these are light-grey limestones with Upper Cretaceous marine fossils, which are exposed in sections north of the Niti Pass, and in other passes across the watershed between the Ganges and the Sutlej at elevations between 16,000 and 18,000 feet above the sea.

Apparently conformable to the Upper Cretaceous strata, there is on the Húndés high plateau a marine Nummulitic formation much disturbed and altered by igneous rocks, and above this, but unconformably, are sandstones resembling the Siwaliks in character, but without fossils. The sandstones are covered by the horizontally stratified beds of Newer Tertiary age, which form the plain of Húndés, extending 120 miles in length by 15 to 60 miles in breadth, at an elevation of about 15,000 feet above the sea. The sections in the ravines of the Sutlej, nearly 3000 feet deep, show that this plain is composed of boulders, gravel, clay, and mud of all degrees of fineness. The only fossils known from these beds are Mammalian bones, which were at first considered to be of Siwalik age, but Lydekker has shown that they belonged to living genera, and that consequently the deposits are of late Pliocene or even Pleistocene age. Griesbach considers the beds to be of lacustrine origin.

The lines of flexures which form the Himalayas are considered by the author to have existed in Palæozoic times; but the great lateral compressions which pushed up the enormous masses of the Central Asian plateau with its fringing rims of mountains were evidently formed after the deposition of the Miocene beds, since these latter are contorted and crushed, whilst they are covered by the nearly horizontal newer Tertiary deposits, just referred to, which are almost unaltered; consequently the crushing must have taken place between the deposition of the Miocene and Pliocene formations.

In Part II. the author gives a description of some of the principal sections, including those of Painkanda (Garhwál) and the Bhót Mahals of Kumaun, with notes on the Central Himalayas between the Kamet Peak and Spiti. A special feature in this Memoir is the number of excellent figures of natural profiles and ideal sections, the latter constructed on the scale of one mile to an inch both for the vertical and horizontal dimensions. To these are added numerous heliographic reproductions of photographs, taken by the author, which afford realistic pictures of the physical and geological features of the regions described, and they show very clearly the wonderful extent of folding and contortion which the rocks have undergone. It may be said that the value and interest of this report is as much due to the well-known artistic ability of the author as to his capacity as an able geological investigator.

II.—Proceedings of the Yorkshire Geological and Polytechnic Society, Vol. XII. Part I. pages 1–130, with 4 Plates. 8vo. (Halifax, 1892.)

THIS comprises some very interesting and useful papers.

I. T. Hick gives a résumé of the latest information about the growth and structure of the *Calamitæ* from the Coal-measures of Yorkshire.

II. B. Holgate, by means of a very careful examination of 60 beds of the Coal-measures at Leeds (Patent Brickyard, No. 1; Patent Brickyard, No. 2; Boyle's Brickyard; and Gould and Stevenson, Hunslet) finds reasons for indicating the original conditions and

mode of origin of the several strata, defining their methods of deposition and their properties. These strata are successively arranged (from below upwards) in a long table (pages 16–21), with (1) their local names; (2) their original constituents, and their included animals and plants; (3) the mode of their formation as deposits; (4) the changes that have taken place in them since their deposition; (5) their present properties and uses; (6) their thicknesses. Thus, taking a series of Mr. Holgate's results in order, it is pointed out that there is indication of (1) a beach; then (2) of dry land of fine siliceous loam: (3) sinking land (with spores, fish-remains, and trees); (4) land still sinking, and the deposit becoming coarser, with the water almost motionless, away from the force of the stream; (5) centre of the stream, water flowing more quickly; (6) direction of river still changing, water with quickening speed; (7) ripple-marks at top, stream changing course and leaving ripple-marks dry; (8) stream again, over dried sandstone, depositing fine white sand, which takes casts of ripple-marks below; (9) centre of stream now at some distance, and water moving gently; (10) water more rapid; (11) water very slow; mud with Anthracosiæ, fish-scales and water-plants; (12) stream quickening, and so on, with ever variable conditions of muds, sands, and coals, with or without Fishes, Anthracosiæ, Lepidostrobi, Stigmariæ, Ferns, Calamites, etc. The salt and mineral waters are referred to; the seat-earths or fire-clays with their included roots and rootlets, are especially noticed (pp. 11–13), with their relation to the trees and plants once rooted in them, and to the coal-beds overlying them. The shales, including the blue, brown, and grey "binds," and the ironstone nodules, some of which yield the "best Yorkshire iron," are also described, both in the Table and at pages 13 and 14.

The evidences of the growth of land and its replacement by water, at this area, usually with river-currents, but once at least forming a lake, with mud full of fishes and molluscs, are interesting, and apparently very judiciously worked out. The chemical action in dissolving and redepositing mineral-salts, and forming concretions and definite nodules are noticed; as well as the effects of lateral thrusts and slidings. Lastly, the appreciations and uses of the several beds, and their measured thicknesses, render this memoir very valuable both to the practical people of Leeds and to geologists in general.

III. C. E. De Rance continues his important researches, with the other members of the Underground Water Committee of the British Association, and here treats of the underground waters in Lincolnshire, as proved by borings at Gainsborough, Worksop, Horncastle (tapping at Woodhall Salim Spring and as shown in the Appendix, pp. 35–51) by the supply from wells and borings in the several geological series of strata, there being about 15 water-bearing horizons, half of which are regarded as good for a public water-supply.

IV. G. R. Vine supplies "Notes on some new little-known Eocene *Polyzoa*," pp. 52–61, mostly from Fareham in Hampshire. The

stratum from which Mr. A. Bell obtained them is regarded by him as "a passage-bed between the London Clay and the Bracklesham series." From the Eocene beds of England Mr. Vine here enumerates 8 species of the *Cyclostomata*; and 21 of the *Cheilostomata*; and he describes 3 of the former, and 4 of the latter group.

V. A. S. Woodward, treating of the "Hybodont and Cestaciont Sharks of the Cretaceous Period," first refers to the characteristics of the *Hybodus* of the Lias and other Mesozoic formations, and proceeds to describe in detail some of the special characters well shown in remains of *Hybodus basanus*, collected from the Wealden beds of the Hasting coast by the late Mr. S. H. Beckles, and now in the British Museum. One of the specimens is shown in pl. i. and another in pl. ii. fig. 1. A dorsal fin-spine of *Synechodon* (?) from the Gault, p. 66, is figured in pl. ii. fig. 2. Some characteristic teeth of *Synechodus Illingworthi* (formerly recognized as *Acrodus* or *Hybodus*) are described pp. 66, 67, and figured pl. ii. figs. 1–7. The author considers that *Drepanephorus*, and some Cretaceous teeth known as *Strophodus* and *Acrodus* belong to *Cestracion* or the Port Jackson Shark; and one of these, *Acrodus rugosus*, he here describes and figures, p. 67, pl. ii. fig. 8.

VI. J. E. Bedford directs attention to "Evidences of Glacial Action near Leeds," on the Ganiston beds of the Lower Carboniferous shales and grits quarried at Headingley, in the valley below Meanwood. He notices moraine material of sandy clay, with irregular patches of sand, and containing great quantities of subangular blocks of grit-rock derived from the Ganiston beds. The shales below have also been much bent and crushed. The probable direction and range of the ice are discussed.

VII. J. S. Tute describes a limestone conglomerate of Permian age at Markington, and a section of Permian beds near Wormald Green.

VIII. G. R. Vine describes (pp. 74–92), and figures in plates iii. and iv. some peculiar Palæozoic Polyozoa, belonging to the genera:—1. *Vinella*, Ulrich; 1 species and variety: 2. *Ascodictyon*, Nicholson and Etheridge, jun.; 7 species: 3. *Rhopalonaria*; 2 species. These appear to be Ctenostamatous *Polyzoa* and early representatives of the Stoloniferous *Vesicularidæ*, or possibly of the *Eutoprocta*. The late Mr. Busk noted that Mr. Vine was the first to indicate this alliance and Mr. Ulrich accepted it, and added the genus *Vinella* to the group. They belong to the Silurian, Devonian, and Carboniferous formations of Britain and America. They consist of creeping or attached stolon-like threads, with numerous vesicles (cells? or zoœcia?), and plate iv. illustrates *Ascodictyum siluriense* and the recent *Valkeria tuberosa* together for comparison of the fossil and recent forms.

IX. R. Reynolds illustrates by diagram the Intense Rainfall at Leeds on July 19th, 1891.

X. J. Spencer, proving "the Affinity of *Dadoxylon* to *Cordaites*," observes (p. 104) that "so long as *Dadoxylon* was regarded as a true Pine, it appears to afford a strong argument against the theory

of evolution; but now that it has been shown that its supposed affinity with the Pines is based entirely upon the resemblance of its wood to that of the Araucaria, the structure of the whole stem, pith, wood, and bark being taken into consideration, it is seen to have more affinity with the Cycads as seen in *Cycas revoluta*, than with *Araucaria*, and it is found to occupy its natural place both in the vegetable kingdom, and in the order of its appearance in geological time, according to the theory of evolution."

XI. E. Jones details the circumstance of the further examinations of the Elbolton Cave, and states that continued exploration is required.

XII. A bibliography of memoirs relating to the geology of Yorkshire for 1889–90 is appended at pages 119–122. T. R. J.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—April 27th, 1892.—Prof. J. W. Judd, F.R.S., Vice-President, in the Chair.—The following communications were read:—

1. "Notes on the Geology of the Northern Etbai or Eastern Desert of Egypt; with an Account of the Emerald Mines." By Ernest A. Floyer, Esq., F.G.S.

The principal feature in the district is a long ridge of igneous upthrust running N.N.W. and S.S.E., in which porphyry rises into lofty peaks, whilst the lower parts are formed of granites and sedimentary rocks. To the west of the watershed, sedimentary rocks occur dipping slightly to the west.

The following succession of rocks in descending order is given by the author:—Limestone, sandstone, clay, 'cataract'-rock (corresponding to the *Stock-granit* of Walther), and compact hard granite. The sedimentary rocks are frequently metamorphosed, and the author states that every stage of metamorphism is shown, from sandstone to compact green granite. The blue clay shows various kinds of metamorphism, and forms the pistachio-breccia containing topazes, and the mica-schist, mica-slate, and talcose blue clay of the mass of Zabbara containing emeralds.

The author discusses certain theoretical questions, and considers that the erosion of the valleys does not indicate the existence of a greater rainfall than the present one. He concludes by giving an account of the emerald mines.

2. "The Rise and Fall of Lake Tanganyika." By Alex. Carson, Esq., B.Sc. (Communicated by R. Kidston, Esq., F.R.S.E., F.G.S.)

In this paper attention is called to certain recorded discrepancies concerning the discharge of Tanganyika by the Lukuja. It is suggested that the rise of the lake is due to the blocking-up of the river by vegetation, assisted by silting during the first rains, whilst the fall is produced by the destruction of the barrier formed in this manner.

II.—May 11th, 1892.—W. H. Hudleston, Esq., M.A., F.R.S.,—President, in the Chair.—The following communications were read:

1. "On the so-called Gneiss of Carboniferous age at Guttannen (Canton Berne, Switzerland)." By Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., V.P.G.S.

It is stated by Dr. Heim (Quarterly Journal, vol. xlvi. p. 237) that the stems of *Calamites* have been found at Guttannen in a variety of gneiss, *i.e.* in one of a group of rocks which exactly "resemble true crystalline schists in mode of occurrence. Petrographically they are related to them by passage rocks; at least the line of separation is not easily distinguished. . . . The Palæozoic formations mostly show an intimate tectonic relation to the crystalline schists, and have been converted petrographically into crystalline schists."

The author describes the result of a visit to the section at Guttannen in company with Mr. J. Eccles, F.G.S. (to whom he is greatly indebted for kind assistance), and of his subsequent study of the specimens then collected. The belt of sericitic "phyllites and gneisses," presumably of Carboniferous age, represented on the Swiss geological map (Blatt xiii.) as infolded, at and above Guttannen, in true crystalline gneissoid rocks, is found on examination to consist partly of true gneisses, partly of detrital rocks. The boulder from which the stems in the Berne Museum were obtained belongs to the latter. These rocks sometimes present macroscopically, and occasionally even microscopically, considerable resemblance to true gneisses, but this proves on careful examination to be illusory. They are, like the Torridon Sandstone of Scotland, or the *Grès feldspathique* of Normandy, composed of a detritus of granitoid or gneissoid rock, which sometimes forms a mosiac resembling the original rock, and which has been generally more or less affected by subsequent pressure and the usual secondary mineral changes. Thus, if the term be employed in the ordinary sense, they are no more gneisses than the rocks of Carboniferous age at Vernayaz (Canton Valais) are mica-schists, but in some cases the imitation is unusually good, and, so far as the author saw, there are at Guttannen neither conglomerates nor slates to betray the imposition, as happens at the other locality.

2. "On the Lithophyses in the Obsidian of the Roche Rosse, Lipari." By Prof. Grenville A. J. Cole, F.G.S., and Gerard W. Butler, Esq., B.A., F.G.S.

The rock described in this paper differs in no essential particular from that at Forgia Vecchia, or from the obsidian on the north flank of Vulcano; but the specimens show in a specially striking manner the passage through various stages of lithophysal structure, from indisputable steam-vesicles with glassy walls to typical solid spherulites. A full description is given of the formation of spherulites by a double process—firstly, divergent growth from the margins of vesicles outwards, and, secondly, convergent growth inwards from the margins towards the centres of the hollows, until in the smallest cases the fibres from the opposite sides of the vesicle may meet in the

centre, producing a spherulite, which, but for the occurrence of intermediate stages, might be supposed to have originated entirely by divergent growth. The authors give details of the appearances presented by intermediate stages of growth.

The prevailing type of spherulite, both in Lipari and Vulcano, shows in section a dusky fibrous central area, which may possess concentric as well as radial structure, surrounded by an irregular brown cloudy zone of various width. The authors' studies lead them to the conclusion that this type owes its characters to the dual mode of growth, and therefore to the original presence of vesicles in the rock. Commonly the process of infilling does not go so far as this; on the ends of the felspar fibres plates of tridymite are deposited, and this seems to close the growth. It is clear that the lithophysal structure of the Lipari obsidians was formed during the cooling of the mass, and not by subsequent amygdaloidal infilling of vesicles.

The authors discuss the effect of confined vapours on such rocks as those forming the subject of the paper, noting that these vapours may be kept at a high temperature for a considerable time, each vesicle thus becoming a sphere of hydrothermal action; so that if the surrounding glass remains at a temperature little below its fusion-point, crystallization will be promoted in it, and at the same time the action of the vapour in the vesicle will produce reactions on its walls.

An Appendix, by Prof. Cole, treats of the lithophyses and hollow spherulites of altered rocks. While admitting the presence of true lithophyses in many of the Welsh lavas, he is not prepared to abandon a former suggestion that the interspaces between successive coats of the Conway lithophyses result from alteration of a formerly solid mass. In the lavas of Esgair-felen and near the Wrekin he has no doubt as to the production of "hollow spherulites" by ordinary processes of decay. The typical Continental pyromerides are truly spherulitic, as is much of the Wrekin lava. In the latter case and that of the rocks of Bouley Bay it will be difficult to distinguish between infilled primary and secondary cavities.

CORRESPONDENCE.

"THE RECENT ELEVATION OF THE HIMALAYAS."

Sir,—The nummulitic limestones occurring in the Hundes Valley show that in Eocene times the ground where the Himalayas now stand was covered by the sea. This is, I believe, admitted by Dr. Blanford. In the Manual of Indian Geology for which he and Mr. Medlicott are responsible he goes further, and expressly says that "*at the close of the Miocene epoch no such mountain barrier as exists at present separated the Indian peninsula from Central Asia*" (op. cit. p. 585). The opinion of Dr. Blanford in 1878 therefore was that the Himalayas are of *post-Miocene* origin.

The superficial beds which contain the mammalian remains we have been disputing about lie unconformably upon certain speckled

sandstones which Griesbach classes as of "Miocene and possibly of later age." Mr. Lydekker, after discussing the remains, says of these mammalian beds, "the beds in question are probably of *Pleistocene age*, and almost certainly not older than Upper Pliocene" (Records, Geol. Surv. India, vol. xiv. p. 181).

General Richard Strachey, who examined them with great care, says of them in his article "Himalaya" in the last edition of the Encyclopædia Britannica, "there is no room for doubt that these deposits have been raised from a comparatively low level to their existing great elevation of upwards of 15,000 feet *since they were laid out*." Mr. Griesbach allows that these beds are everywhere raised up on end as is also the case along the southern margin of the lower hills which are skirted by the Siwaliks (Mems. Geol. Survey of India, xxiii. p. 34).

These facts seem to me to justify my contention that the Himalayas have been largely uplifted since Pliocene times. Mr. Blanford was himself once of the same opinion. When the Manual already quoted was published, namely, in 1878, his view was that the Himalayan elevation took place after the deposition of the Siwalik beds, that is, that it was post-Pliocene. I understand him to say that he has since changed his mind because Mr. Lydekker has shown the Rhinoceros remains found at Hundes to belong to an extinct genus, and he refers me to the Catalogue of Fossil Mammalia in the British Museum, vol. iii. p. 158, where he says I shall find that the Tibetan Rhinoceros was closely allied to a species belonging to *Aceratherium*, a small hornless (extinct) group with well-developed incisors in both jaws, and Dr. Blanford accordingly says very positively that the Rhinoceros in question cannot have been the *R. antiquitatis* as I had conjectured, and implies that the beds in which the remains occur must belong to an older horizon.

I confess, Sir, that his statement surprised me greatly. In the first place I knew that the remains from Hundes comprised no teeth and no skull, and I could not understand on what possible grounds Mr. Lydekker, whose patience and caution are so conspicuous, could have come to such a very definite conclusion about the specific character of the Rhinoceros referred to. As a matter of fact he does nothing of the kind. He expressly says of the remains "*they are not specifically determinable*" (loc. cit.).

In the face of this specific statement, I can neither understand how Dr. Blanford should have attributed to Mr. Lydekker a conclusion for which there seems to be no foundation, and secondly, how he can, because of a mere phantasm, justify to himself and us completely changing his own views on a most critical matter. I am bound to say, apart from everything else, there is a very strong *a priori* improbability that the Rhinoceros remains from Hundes belong to an extinct genus. Mr. Lydekker distinctly says "all the remains found at Hundes, with the doubtful exception of *Hippotherium*, belong to *living* genera," and he treats them as Pleistocene or Pliocene. The genus *Aceratherium* belongs to an older horizon altogether. So far as we know it was a tropical or

semitropical genus, and there is no warrant for attributing to it, as Dr. Blanford does, a capacity for living under conditions like those of the Hundes plateau. The only species of Rhinoceros known to me which were the companions of the Horse, etc., etc., elsewhere, were the *R. antiquitatis* and the *R. Merckii*, to one of which I believe the remains probably belonged.

My view in regard to the impossibility of supposing that any species of Rhinoceros could live where the remains are found in Tibet is shared by better authorities than myself. Strachey expressly says that "their existence in the present condition of the Tibetan plateau would be quite impossible," while Dr. Falconer, *facile princeps* as an authority on the Pachydermata recent and fossil, says, "Henry Colebrooke, the first who, along with Colonel Crawford, measured the heights of the Dwalagiri, procured from the plateau of Chauthan in the Himalayas, at a height of 17,000 feet above the sea-level, fossil bones, which were brought down and exported as charms into India, to which the natives attributed a supernatural origin, and called them 'lightning or thunder bones.' At the present time, during eight months in the year, the climate differs in no important respect from that of the Arctic circle, and in the whole of the district there is not a single tree or shrub that grows larger than a little willow about nine inches high. The grasses which grow there are limited in number, and the fodder in the shape of Dicotyledonous plants is equally scarce. Yet, notwithstanding this scantiness of vegetation, large fossils were found of the Rhinoceros, the Horse, the Buffalo, the Antelope, and of several carnivorous animals; the group of fossil faunas as a whole involving the condition that, at no very remote period of time, a plateau in the Himalayan Mountains, now at an elevation exceeding three miles above the level of the sea, where we get the climate of the Arctic regions, had then such a climate as enabled the Rhinoceros and several subtropical forms to exist. . . . . The only rational solution which science can suggest is that within a comparatively modern period, a period closely trenching upon the time when man made his appearance upon the face of the earth, the Himalayas have been thrown up by an increment closely approaching 8,000 or 10,000" (Proc. Roy. Geol. Soc. vol. viii. pp. 41 and 42). I commend this passage to Dr. Blanford.

I had written a detailed criticism of his rejoinder, in which I traversed every point he has made, but I do not think it right to unduly load your pages with an ephemeral polemic, and I have merely therefore selected one issue as a sample, and I venture to think it shows that the position I have supported is unassailable.

HENRY H. HOWORTH.

CONE-IN-CONE STRUCTURE.

SIR,—In reference to the statement of Mr. Alfred Harker, F.G.S., regarding radiation, and inversion of the cone structure, in nodular masses, in May Number of GEOL. MAG., I hope you will kindly allow me to state, that I have in my printed paper, on "Cone-in-

cone Structure," mentioned in March Number of GEOL. MAG., offered a short explanation, that seems satisfactory to myself, and others, that have seen my specimens,—regarding the radiated and inverted structure of the cones, sometimes seen in nodular masses; this radiation, as stated in paper, pp. 26, 27, being due to secondary causes that had acted on the cone stratum after the cone structure itself had been developed.

I point out in my paper, in the first place, that the cones, in any continuous level stratum, were evidently formed, through the upward escape of gases from below, whilst the stratum itself was being deposited the cones, invariably having their apices directed downwards to the lower part of the bed. In those cone strata where there has been an after-tendency in the sediments to aggregate into nodular masses, these nodules, in their contraction from larger into smaller dimensions, during their solidification, often show clear evidence of the gradual pulling of the cones, from their former erect position, all over the surface of the nodules; they radiating, outwards, from the centre to the circumference, there also being evidence of much crushing and distortion of the cone structure all along their outer edges. I further point out that "where the contraction would be greatest, as in the more argillaceous nodules, there may have been a bending, and in some instances a complete inversion of the cones around the edges of the nodules."

Since my paper was printed, in 1886, I have obtained many other illustrative specimens from our Scottish coal-field. These clearly show that the radiation and inversion of the cones, in nodular masses, was due to after secondary causes, the cone structure being first, the formation of the nodules being second, and the amount of radiation, and inversion of the cones, affords a measure of evidence as to the amount of contraction that has taken place amongst these nodules previous to complete solidification.

HUNTERIAN MUSEUM, UNIVERSITY, GLASGOW.
May 9th, 1892. JOHN YOUNG.

DISSIPATION OF ENERGY AS A GEOLOGICAL FACTOR.

SIR,—Many readers of the GEOLOGICAL MAGAZINE will, perhaps, be glad to have their attention drawn to to the following passage, which concludes an article by Lord Kelvin (P.R.S.), on "Dissipation of Energy," in the "Fortnightly Review" for March, 1892:—

"The whole store of energy now in the sun, whether of actual heat, corresponding to the sun's high temperature, or of potential energy (as of a not run-down weight of clockwork)—potential energy of gravitation depending on the extent of future shrinkage which the sun is destined to experience, is essentially finite; and there is much less of it now than there was three hundred thousand years ago. Similar considerations of action on a vastly smaller scale are, of course, applicable to terrestrial plutonic energy, and thoroughly dispose of the *terrestrial 'perpetual motion,'* by which Lyell and other followers of Hutton, on as sound principles as those of the humblest mechanical perpetual-motionist, tried to find that

the earth can go on for ever as it is, illuminated by the sun from infinity of time past to infinity of time future, always a habitation for race after race of plants and animals, built on the ruins of the habitations of preceding races of plants and animals. The doctrine of the 'Dissipation of Energy' forces upon us the conclusion that within a finite period of time past the earth must have been, and within a finite period of time to come must again be, unfit for the habitation of man as at present constituted, unless operations have been, and are to be, performed, which are impossible under the laws governing the known operations going on at present in the material world."

There can be no necessity for pointing out the importance of this *dictum* from the pen of Lord Kelvin; it supports my own contention in the GEOLOGICAL MAGAZINE of July and October, 1891 (pp. 300 and 479-80). I will not intrude upon your space by reiterating what I have already put into print, but I trust you will, with your usual courtesy, allow me to refer the reader to such passages as are to be found in my little work.[1] In the light of what I have quoted above from Lord Kelvin it can scarcely be said that I spoke too strongly in animadversion on the Huttonian School, in the concluding paragraph of my "Note on the Airolo Schists Controversy" in 1890 (See GEOL. MAG. Dec. III. Vol. VII. p. 259).

The concluding paragraph of Sir A. Geikie's Presidential Address to the Geological Society for 1892 shows how opinion is veering at the present moment; and during the present session two papers of importance have appeared, one by Professor Bonney and Gen. MacMahon, another by Messrs. Dakins and Teall, in which attempts have been made to work out the history of the structural phenomena observable in igneous masses of particular areas on principles applicable to an universal magma, at a period of the Earth's history when the energy since dissipated by radiation into space was concentrated in the lithosphere. A great deal of what the writers referred to have now put forward was seen more than forty years ago by that sagacious geologist, the late Prof. John Phillips, F.R.S., as applicable to the crystalline rocks of the Malvern range (see Mem. Geol. Survey, vol. ii. part 1), which he saw, with an insight not befogged by the later mists of "regional metamorphism," to be in the main a truly igneous series. On the Malvern Crystallines I hope, after ten weeks' hammering at them, to have more to say anon.

A. IRVING.

WELLINGTON COLLEGE, BERKS,
11*th May*, 1892.

EARTHQUAKE SOUNDS.

SIR,—There are one or two points in Mr. C. Davison's paper on earth-quake-sounds I should like to draw attention to.

In most Italian tectonic earthquakes, the sound phenomena precede the mechanical disturbances, though the former overlap the latter the nearer the epicentrum is approached. This means that

[1] "Metamorphism of Rocks" (London, 1889), see pp. 18, 19, 22, 23, 70, 71, 94, 95, and 96.

their production is almost simultaneous, but the smaller sound vibrations travel at a greater rate than the larger mechanical ones.

The fact that the more destructive the earthquake, the less marked proportionally is the intensity of the sounds is easily explicable. The sound vibrations are more quickly used up in traversing a given thickness of rock, whilst the mechanical vibrations have hardly been influenced in the short distance travelled in the shallow focussed shocks that constitute the majority of the destructive earthquakes. For the same reason of the more rapid destruction of the sound vibrations by the rocks traversed the seismic area of sounds is much more limited than that of the quakes. It must also be remembered that during destructive earthquakes much of the noise is due to cracking and falling buildings, shaking trees, etc.

As to the cause of earthquake-sounds I believe they are very various in different earthquakes, and even in any one earthquake. Mr. Davison speaks only of fault friction, but rather neglects the actual initial fracture, which we should expect would produce a very loud noise. Next comes rock-crushing, such a common phenomenon in any mountain region, especially along the central ridges and troughs of anticlines and synclines. Then again we have to consider the fracturing or splitting of rock by the formation of igneous dykes, which may occur in a region free from surface volcanic phenomena. Is it possible that the hundreds of dykes that rent the old rocks of the northern counties of England and Scotland, and most of which never reached the surface, were not accompanied in their formation by earthquakes and *earth-sounds*.

The origin of these sounds is no doubt the smaller vibrations produced by the mechanical disturbances in fracturing and slipping or grating in the tectonic earthquakes. In the case of volcanic or plutonic shocks the sound is in the first place due to splitting and fracturing of the solid rocks. It is then followed by the friction of the injected fluid magma, and the sudden sharp arrest of this against the walls of the cleft. The phenomenon is very similar to the sounds produced by suddenly pumping water into a collapsed leather hose-pipe, closed at the opposite outlet. We have in such a case first a gentle rush followed by a sharp snack as the water is arrested by the fully distended walls. Very similar sound-phenomena may be heard on closing sharply a tap through which water, under considerable pressure, is flowing. There is yet another source of sound in such earthquakes, and that is the vesiculation of any aquiferous magma when allowed to expand through a newly formed fissure just filled by it.[1] All these sounds are practically simultaneously produced, and their combined effect with the predominance of one or another would explain the variable nature of the audible phenomena of an earthquake.

The mechanism of production of earthquake sounds I fully discussed years ago,[2] whilst experimental researches on this question

[1] This may possibly explain the boiling cauldron sound so often mentioned in earthquake descriptions.

[2] See my monograph of the Earthquakes of Ischia, pp. 82, 89, and Proceed. Roy. Soc. Dublin, 1886, pp. 120, 124.

have been published not long since by Italian investigators, who have shown the conclusions arrived at by myself and others were correct, that sound-waves travel faster than the coarser mechanical vibrations when traversing most rocks.

7, Chiatamone, Naples. H. J. Johnston-Lavis.

THE TEMPLE OF JUPITER SERAPIS IN PUTEOLI (POZZUOLI).

Sir,—It is well known that the ruins of this Temple have been looked upon as the most striking example of subsidence in historic times. Although it has taken place within the Christian era, the date has been but vaguely known. Babbage, in his article, Geological Transactions, vol. iii. (1847), mentions an inscription of Alexander Severus on the Temple asserting it to have been adorned by his munificence. As Alexander Severus reigned from A.D. 222 to 235, at that time the Temple must still have have been above sea-level. In Lyell's Principles, vol. ii. p. 173, there is a quotation from Loffrado which proves that in 1530 a great part of the site of modern Pozzuoli of ancient Puteoli, was under water. The city was captured by Alaric A.D. 410; then by Genserie 455; then by Votila 545 (E. H. Bunbury in Smith's Dict. Geog. art. Puteoli); but we have no information as to whether the Serapeum was then above or under water. The Temple of Serapis then was above water in 230 and below water 1530, and during the intervening 1300 years there seems no reliable information.

However, in the *Acta Reta et Pauli*, Greek forms, dating according to Lipsius from the fifth century, we have the following passage—I quote from Walker's Translation Ante-Nicene Library, vol. xvi. p. 258:—"And Paul being in Ponteole (Puteoli) and having heard that Dioscorus had been beheaded, being grieved with great grief gazing into the night of Heaven said 'Oh Lord Almighty . . . . punish this city and bring out of it all who have believed in God and followed His word.' He said to them therefore 'follow me.' And going forth from Pontiole they came to a place called Baias (Baiae) and looking up with their eyes they all see that city Pontiole sink into sea-shore (*εἰς τὴν ὄχθαν τῆς θαλάσσης*) about one fathom (*ὡσεὶ ὀργυίαν μίαν*) and there it is until this day for a remembrance under the sea." It is evident that when the Greek of the *Acta Petri et Pauli* was written Pozzuoli was under water, as it was in the days of Loffredo (though perhaps not so deeply submerged), and had been so for so long that the memory of the subsidence and the circumstances attending it had been utterly lost. If we allow a century to have been sufficient to have caused this utter oblivion, we have then reduced the 1300 years to about 150. In other words somewhere between the middle of the third century and the middle of the fourth this event must have occurred. The phrase "into the sea-shore" (*εἰς τὴν ὄχθαν τῆς θαλάσσης*) supports Babbage's theory that the Temple first sank in a lake of brackish water. This is confirmed by the assertion that the city sank a fathom (*ὡσεὶ ὀργυίαν μίαν*).

J. E. H. Thomson.

10, Allen Park, Stirling.

OBITUARY.

WILLIAM REED, M.R.C.S. (ENGL.), F.G.S., Etc.

Born 1810. Died 1892.

By the death of Mr. William Reed, of Blake Street, York, the Yorkshire Philosophical Society has lost one of its oldest members, and certainly—as regards its Museum—one of its most liberal benefactors.

Mr. Reed commenced life as pupil of Mr. Ness, surgeon, Helmsley, and it was probably during his residence in that district that he acquired that strong affection for the study of geology which characterized his later years, and gave him a place amongst the first geologists of the past half century. Upon leaving Helmsley he entered St. George's Hospital, London, and subsequently continued his studies at Paris. In the year 1837 he qualified as licentiate of the Society of Apothecaries, and the following year he took his degree as member of the Royal College of Surgeons (Engl.). He was afterwards appointed resident medical officer of the York County Hospital, and surgeon of the York Eye Institution, founded by Mr. Henry Russell. After occupying these positions for some years, to the great benefit of the patients under his care, he removed from York to Foston, and there carried on a private practice with marked success for several years. Afterwards he again took up his residence in York, and remained there during the rest of his life. In York he entered into partnership with Mr. Benjamin Dodsworth, who at that time enjoyed a very extensive practice, and the partnership continued several years. After leaving his partner Mr. Reed carried on practice for some years alone. This practice, owing to his great diligence and professional skill, became very extensive, so much so, indeed, that he found it necessary to take a partner, and he was joined by Mr. Rose, with whom he worked until nine or ten years ago, when Mr. Reed retired from the profession. Since that time he has devoted his leisure to the study of geology, to which a great portion of his earlier leisure time had also been given. Many years ago he became connected with the Yorkshire Philosophical Society, and it is to him more than to any other person that the Museum at York owes its present high standing. His whole soul was devoted to the science of geology, and the excellent collections of specimens now to be seen in the Museum will cause his name to be handed down to many future generations, and will testify to his great liberality. For many years he has continually been adding geological specimens to the Museum, but his liberality is most apparent in the two collections of specimens which he presented, which have raised the Museum to the first position in the country. The first of these collections was presented in 1878. It contained about 100,000 geological specimens collected by Mr. Reed himself. The following is an extract from the Yorkshire Philosophical Society's annual report:—"In the Geological Department, the Council have formally to announce the presentation

to the Society of the valuable geological collection of their respected Vice-President, William Reed, Esq., F.G.S. The collection presented by Mr. Reed has been formed at a great cost over a period of many years, and has been well known to geologists as one of the most valuable private collections in the United Kingdom. The Council congratulate the Society on its possession, as tending to raise the Museum to the first rank among similar scientific Institutions in this country."

The collection presented by Mr. Reed consists of:—1. A complete set of shells of the land, freshwater, and marine mollusca of Great Britain, comprising several forms first ascertained to be still living members of the British Fauna during the dredging expedition of the "Lightning" and "Porcupine." 2. An extensive collection of mammalian remains from English Post-Tertiary deposits, remarkable among which, for their fine state of preservation, are the teeth and bones of Rhinoceros, Horse, Hippopotamus, Urus, Megaceros, Elephant, Bear, Lion, Hyæna, Beaver, etc. 3. A large series of shells of the same period, from fluviatile and marine deposits, in various parts of England, Scotland, and Ireland. 4. A magnificent collection of fossils from the Norwich and Coralline Crags. The suite of vertebrate remains, especially, is of great value. This is probably the finest private collection of Crag fossils in England, and it is doubtful whether it can be equalled in any of our great public museums. 5. A fine series of plant remains from the beds of Bovey Tracey, Mull, and Antrim. A collection of Miocene shells from the neighbourhood of Bordeaux and Cannes. 6. A large collection of Eocene fossils in a beautiful state of preservation, in which the several subdivisions of the deposits of that period in England are fully represented. 7. An extensive assemblage of fossils from the Chalk, Greensand, Gault, Neocomian, and Wealden. 8. A very large and valuable series of Jurassic fossils. 9. A series of British Palæozoic fossils, especially rich in Carboniferous limestone fossils from the neighbourhood of Settle (upwards of 200 species). A most important feature from a scientific point of view in Mr. Reed's collection is the great care which has been taken to indicate, by labels, the exact locality from which the several specimens have been obtained, so that thorough reliance may be placed on them.

In December, 1880, Mr. Reed made a second presentation to the Society. This consisted of a collection of specimens formed by the late Mr. Edward Wood, F.G.S., of Richmond, Yorkshire, and since known as the "Wood Collection." This, although by no means equal to the one previously presented, is yet a collection of great value to geologists, being particularly rich in Yorkshire fossils. Four great collections had up to that date been formed in Yorkshire during the last half century—namely, those of Mr. Bean and Mr. Leckenby, of Scarborough; Mr. Wood, of Richmond; and Mr. Reed, of York. Of these, a considerable portion of Mr. Bean's fossils were purchased by the Yorkshire Philosophical Society in 1860 for £200, and two of the other collections—Mr. Wood's and Mr. Reed's—have by the public-spiritedness and liberality of the last-named gentlemen

found their place in York Museum. The Leckenby collection is now in the Cambridge Museum. Mr. Edward Wood had his home among the hills of North-west Yorkshire, and from the Mountain Limestone and contiguous strata of that district he formed the collection which, added to that formerly presented to the Museum by Mr. Reed, raised that Museum to a high position among the geological museums of this country. The Society is greatly indebted to Mr. W. Reed for his presentation of these valuable collections, and also for the able service which for two years he rendered as honorary curator in the arrangement of the collections.

Mr. W. Keeping, M.A., a former keeper of the Museum, read a paper at the monthly meeting of the Yorkshire Philosophical Society held in January, 1881, upon the "Wood Collection." In it he says: —The collection of fossils formed by the late Mr. Edward Wood, F.G.S., of Richmond, Yorkshire, is the result of the constant attention and labour of more than 30 years of his lifetime. Living in a district rich in some of the most beautiful and attractive of fossil organic remains, and impelled by a strong natural love for palæontology, Mr. Wood became an ardent collector of all specimens of geological interest, and such was his success that he ultimately became distinguished as the possessor of one of the finest private geological collections in Britain. Naturally this collection is particularly rich in objects from the Yorkshire dales, especially his own dale, Swaledale: but it also includes collections from many other British localities which were obtained by the help of his many scientific friends and acquaintances, in his own travels, or by his own purchases. Thus the collection came to spread over a wide area both in space and time, forming a fair representation of the whole of the geological periods, but specially rich and valuable in certain formations. To the York Museum this collection is particularly valuable, for it is precisely where we were poor that we here find the greatest riches. It was in the Permian, Coal-measures, the Carboniferous Limestone, and the Old Red Sandstone that our collection, including Mr. Reed's original museum, was weakest; while in the Edward Wood collection these groups are most perfectly represented. Mr. W. Reed, F.G.S., our honorary Curator of geology, was already acquainted with Mr. Wood's collection, and knew how important an addition it would be to the Society's Museum, and he therefore, as soon as the way to its acquisition was open to him, at once decided to purchase the collection and present it to the Society. As a private collection of Carboniferous Limestone fossils Mr. Wood's museum has never been equalled in England, and the other groups of the Upper Palæozoic rocks are also particularly fine. It is without doubt in the Carboniferous Echinoderms, especially the Crinoids, that the collection is most remarkable, and it is best known to geologists as containing a magnificent series of those Crinoids or "Sea Lilies" named in honour of their discoverer, *Woodocrinus*. Some hundreds of specimens of this beautiful fossil were obtained by Mr. Wood, the duplicates being liberally distributed throughout the various Museums of Europe, while some 80 slabs, including all the

more choice examples, remain in the collection. Many of the slabs include several heads, some of them exhibiting as many as nine distinct individuals, so that we have altogether a perfect forest of these beautiful sea lilies. The spot where these were found is in a quarry at Lymmas House, Holgate, near Marske, a place in Swaledale, some 13 miles from Richmond, and rather difficult of access. They have not to my knowledge been found in any other locality. Altogether the collection numbers over 10,000 specimens; there being, according to a catalogue made by Dr. Henry Woodward, 9365 selected specimens in the cabinets.

Mr. Reed was of a retiring disposition, and took little interest in public affairs. He was, however, of a genial disposition, and in private life was a pleasant companion.

He never married and lived an extremely abstemious life, performing many acts of private charity and benevolence which the world saw not. He suffered a long time from bronchitis, and about ten days before his death he received a chill which brought on his old complaint, to which he succumbed on Monday the 9th of May at the advanced age of eighty-two years.

Mr. Reed's death will be sincerely mourned by many friends, his loss to the city of York will be great, but to the Yorkshire Philosophical Society still greater.—*Yorkshire Herald*, May 10, 1892.

MISCELLANEOUS.

Major John Plant, F.G.S.

The fact that Major Plant is about to sever his long connexion of forty-two years with the Peel Park Museum, Salford, affords us a welcome opportunity of giving a brief sketch of the life and work of one to whom naturalists and the general public of Salford owe so much, and whose long services the Museum Committee recently recognized by a gratifying tribute.

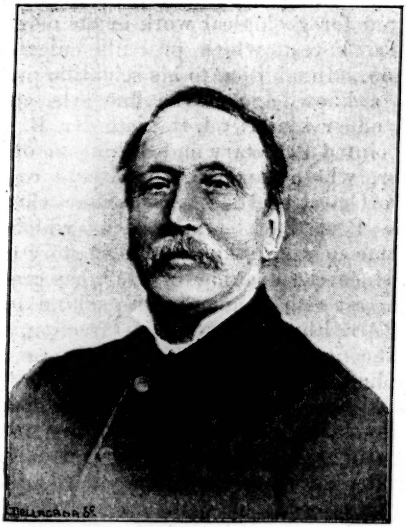
Major Plant is the son of the late Mr. Robert Fisher Plant, a stationer of Leicester, in which town he was born in October, 1819. At an early age he entered the National School of his native town, and there acquired the rudiments of a sound education, side by side with the Right Hon. A. J. Mundella. On leaving that school, he continued his studies at the Mechanics' Institution, displaying considerable taste for drawing and Natural Science. It was intended that he should adopt the medical profession, and with that view he was articled to a surgeon of Leicester, Mr. T. Paget, but it was afterwards found necessary for him to abandon this pursuit, to assist in his father's growing business. In 1844, he was elected Honorary Secretary of the Leicester Naturalists' Club, and shortly afterwards was appointed Curator of a small museum which had been founded in the town through the instrumentality of the Literary and Philosophical Society. By this time Mr. Plant had obtained that keen predilection for geology which has characterized him all his life; and it was also in 1844 that he read before the British Association,

at Birmingham, a paper on "The Discovery of Fossiliferous Keuper Sandstones at Leicester," which were thenceforth included as a distinct stratum in the maps of the Geological Survey.

Doubtless, this scientific taste was fostered by the favourable opportunities offered for geological work in the neighbouring classic area of Charnwood Forest, which presents enigmas yet awaiting satisfactory solution. In addition to his scientific pursuits, he found time to cultivate a knowledge of the fine arts, attaining to some skill in drawing under his friend, the late Mr. B. R. Haydon. In 1846, he was appointed Secretary and Librarian of the Permanent Library, Leicester, where he re-arranged and catalogued 10,000 volumes; and in October, 1849, he became Curator and Chief Librarian of the Peel Park Museum, in which connexion his labours have become so widely known. The story of the Peel Park Museum, which, since the opening in 1849, has gradually grown to be one of the largest establishments of its kind in the country, is also the story of Mr. Plant. Under his direction, and with the aid of generous munificence, it has developed into one of the principal attractions of the busy centre to which it belongs. The library now contains some 60,000 volumes, in place of the 5000 with which it started, and is provided with a handsome reading room. The rooms in which the scientific exhibits are displayed are numerous and well adapted to their purposes; while the art galleries include the great Langworthy Gallery, occupied with marble statues and fine oil paintings of the modern English and French schools. Mr. Plant's varied training had specially fitted him for the management of an institution of such complex character; and now, in its enlarged proportions, altogether beyond the satisfactory control of any one man, there is abundant evidence of his extensive knowledge and skill—signs which only the educated eye can appreciate at their full meaning.

It is, however, principally to Geology and Palæontology that Mr. Plant has given his attention. In 1851 he became a member of the Manchester Geological Society, of which he is now the oldest living member; and in 1864 he was elected a Fellow of the Geological Society of London. About 1870, he began to make a special study of the Coal-measure Fishes in the neighbourhood of Manchester, and his extensive collection has formed the basis of important researches by Dr. R. H. Traquair, of Edinburgh. Several of the species have been named after Mr. Plant. In the Geology of North Wales he has for many years taken a deep interest, especially in the Cambrian fossils found in the locality of Dolgelly. The whole of his collection from this district has been formed under circumstances which make the specimens almost unique, and a selection of the typical forms has been deposited in the British Museum. The type specimen of *Olenus Planti*, named by Mr. Salter and also described by Professor M'Coy in Sedgwick's Catalogue of the Woodwardian Museum, Cambridge, is included in the series. We understand that Mr. Plant is still engaged on researches in the Geology of the South West Coast of Anglesea, the progress of which will be greatly

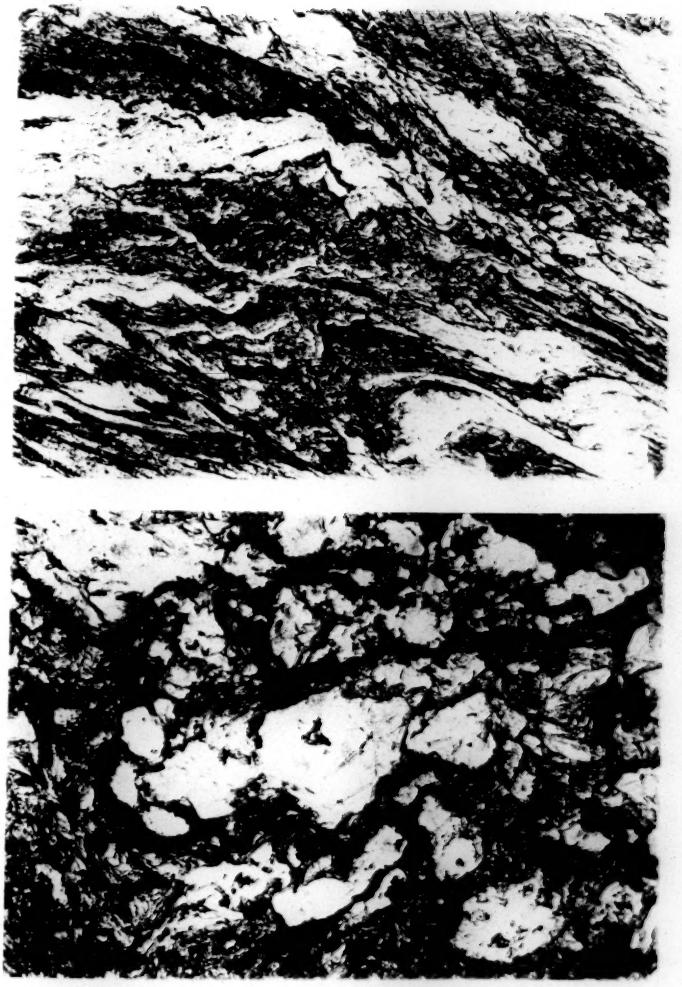
facilitated by his residence in that island, where he now intends to take up his permanent abode, and whither he has for the last twenty-five years resorted for his annual holiday.



Although now in his 73rd year, Mr. Plant bears but little appearance of his advanced age, and in spite of this and a serious fall which he experienced some fourteen months ago, he still retains the ruddy complexion, robustness of body, and cheerful *bonhomie* which have always characterized him. The limits of space prevent our saying more, beyond recording that Mr. Plant has been a member of the 3rd L.R.F., or Salford Corps of Volunteers, since 1859, and in this he now holds the rank of major. He is not the only member of his family who has devoted himself to scientific pursuits, his eldest brother, Mr. James Plant, being the well-known geologist of Leicester, while another brother, Francis, passed some five years in the active investigation of the natural history of Madagascar, where he died a victim to smallpox. Another brother, Nathaniel, also distinguished himself still more by his geological researches, of sixteen years' duration, in Brazil, including the exploration of the Candiota Coal-fields, made on behalf of the Brazilian Government.[1]

Birth of a New Scientific Society.—The New Institute of Mining and Metallurgy, which already numbers more than 100 Members and Associates, recently held its Inaugural Meeting in the Theatre of the Museum of Practical Geology, Jermyn Street, under the Presidency of George Seymour, Esq., A.R.S.M., M.Inst.C.E., F.G.S., ably supported by the leading members of the Mining and Metallurgical profession. The President delivered a most able address, and later in the evening the members and their friends supped at the "Criterion," where speeches were delivered, Mr. F. W. Rudler, F.G.S., making a most brilliant address. We wish the new Society every possible success and prosperity.

[1] See Geol. Mag. Vol. VI. 1869, pp. 147–156, Pls. V.–VI. with description of the Plants by William Carruthers, F.R.S.



To illustrate Mr. Hunt's Paper on the Rocks of South Devon.

THE

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No. VI.—JULY, 1892.

ORIGINAL ARTICLES.

I.—On Certain Affinities Between the Devonian Rocks of South Devon and the Metamorphic Schists.

(Part II.)

By A. R. Hunt, M.A.

(*Continued from page* 247.)

(PLATE VIII.)

The Metamorphic Green Rocks and Devonian Volcanics.

In comparing the quartz-schists with the Devonian sandstones, or grit-bands, several original minerals can be traced from the one to the other, *e.g.* tourmaline, mica, and at least two quartzes. In the case of the Green Rocks and Devonian Volcanics we are met at the outset with the difficulty, that not a grain or crystal of an original mineral has been recorded with confidence as occurring in the metamorphic Green Rocks; though there seems to be little doubt that the latter are often metamorphosed diabases.

The metamorphic Green Rocks seem to be exclusively composed of secondary minerals, of which felspar, hornblende both fibrous and compact, chlorite, and epidote, are the most important.

The Devonian Volcanics, when hard enough for slicing, are for the most part diabases, in which secondary felspar, the two hornblendes, epidote, and chlorite, are here and there developed, to a greater or less extent, by chemical or dynamic action, or by both combined. Microscopic research is thus necessarily limited to the above secondary minerals.

The relation of the Devonian Volcanics to the sedimentary rocks, the relation of the metamorphic green rocks to the mica-schists, and the relation of the Devonian rocks to the metamorphic rocks, from the stratigraphical point of view, form no part of the present inquiry. Suffice it to say that from the latitude of Dartmouth to Torcross, diabases of Devonian age occur here and there striking roughly east and west; and that on crossing the metamorphic border-land we find the place of these diabases taken by green rocks of disputed age.[1]

Within the limits of Dartmouth Harbour and Range we may notice three types of diabase. A porphyritic diabase occurs at

[1] Maps indicating the lines of strike of certain of the Diabases and Green Rocks were exhibited by Mr. W. A. E. Ussher in Section C at the Cardiff Meeting of the British Association, 1891.

Sandquay. The western Blackstone is composed of a fine-grained diabase; while a coarsely crystallized diabase forms the southern and larger portion of the islet known as the Mewstone. The Mewstone rock, or one much like it, appears on the coast between Dartmouth and Blackpool Sands, and again further west inland. The Blackstone (western) rock re-appears in Blackpool Valley in a schistose form, where it resembles a schistose rock occurring between Modbury and Aveton Giffard; while a porphyritic diabase, recalling Sandquay, occurs in the Torcross line, near East Charleton. This variety among the diabases is well matched among the metamorphic green rocks, where at one place we may meet with a fine-grained hornblende schist; at another, a rock in which layers of felspar are the most conspicuous feature; or, in which granular felspar is embedded in a matrix chiefly composed of chlorite; or, in which the felspar grains rest in a matrix in which fibrous hornblende is prominent.

In a quarry of greenstone, west of Winslade, a quarried block was seen to be composed of two distinct rocks closely united; one a pale greyish-green diabase, the other a much darker and more schistose rock which recalled some of the green metamorphic rocks further south, *e.g.* at Bickerton. They will be described as Winslade A (No. 38) and Winslade B (No. 39). In A the original augite crystals are well represented; in B it is doubtful whether any can be recognized. In B there is abundance of green typical chlorite, polarizing a deep blue; in A there is a corresponding mineral of the faintest tinge of green, which Mr. Teall informs me is also probably chlorite. The frequent association and intimate mixture of hornblende and chlorite in both the diabases and metamorphic rocks is noticeable.

It is not difficult to trace the two hornblendes and chlorite from the diabases into the metamorphic rocks. In a vein in the Sandquay diabase (a rock in which the original minerals are nearly obliterated) we have secondary felspar associated with fibrous hornblende and very pale chlorite. At Blackpool, chlorite is intimately associated with compact hornblende. At Winslade we have an occasional crystal of compact hornblende and much chlorite in company with a few streaks of fibrous hornblende. Near Start Point we meet with a highly felspathic green rock of gneissoid character containing compact hornblende, much fibrous hornblende, and chlorite. This rock is an epitome of the decomposition products of the Sandquay and Blackpool valley rocks, the fibrous hornblende and secondary felspar of the one, with the compact hornblende of the other, and the chlorite of both, being all represented near the Start.

An interesting feature in the metamorphic green-rocks is the frequent occurrence of rounded granules of felspar full of greenish belonites (actinolite?) in a matrix of what appears to be chlorite and hornblende in varying proportions. In specimens from near Bickerton, both north and south of the valley, as also in one from Rickham Sands, the matrix is chiefly chlorite; whereas in another from near West Bolbury in which the felspar is less conspicuously

rounded the matrix contains much fibrous hornblende. The belonites in felspar are such a prominent feature in these rocks that I have carefully searched the more chloritic of the Devonian diabases for any indication of the incipient formation of such microliths in felspar, but hitherto without success. In the more altered Winslade rock, B, there are several clear grains with belonites; but, on comparing this slide with the one from Rickham Sands, Mr. Teall considered the grains in the diabase to be quartz, but was unable to detect quartz in the metamorphic rock; twelve grains taken at random proving to be in each case felspar.

Professor Bonney has, however, described quartz with belonites from the Prawle district, associated with a mineral supposed to be kyanite[1] (Q.J.G.S., vol. xl. p. 17). Miss Raisin has also described "a typical slide of chlorite schist," as containing grains with belonites, the grains being attributed to quartz (Q.J.G.S., vol. xliii. p. 719).

My evidence on this point is merely negative. Quartz grains with belonites appear to be absent from my slides of the green rocks, the water-clear grains being felspar. If such quartz-grains do occur, and we have the evidence of Prof. Bonney and Miss Raisin that they do, it is possible that the early stage of their formation may be revealed in the Winslade Devonian diabase referred to above.

Felspar and Felspathic Veins.

In 1839 Sir Henry de la Beche reported the occurrence of gneiss near the Prawle Point, by the addition of felspar to the mica and quartz of the ordinary schist.[2] This statement has been the source of much perplexity to geologists. In 1881 the late Mr. E. B. Tawney declared, "there was nothing approaching gneiss there."[3] In 1883 Prof. Bonney suggested that a mica-schist banded with thin *quartzose* laminæ might be De la Beche's gneiss.[4] In 1891 Mr. W. A. E. Ussher called attention to a grey rock near the Start with incipient foliation, presenting a slightly gneissoid appearance.[5] Messrs. Sedgwick, Murchison and Pengelly have each in turn expressed their doubts as to the occurrence of gneiss near the Prawle.[6]

Sir Henry de la Beche defines precisely the rock he considers gneiss, viz. a rock composed of mica quartz and felspar. Thus Prof. Bonney's quartz-schist will not meet the case for lack of felspar, just as the grey gneissoid rock near the Start[7] must fail for lack of quartz.

The importance of De la Beche's observation consists in the recognition of felspar in the mica- and quartz-schists. This felspar occurs either as laminæ in the schists, or as veins, the latter being sometimes associated with quartz and chlorite.

[1] I regret to say that my own identification of kyanite in the Bolt Schist (Trans. Dev. Assoc. vol. xxi. p. 260) was wrong. I am authoritatively informed that there is no kyanite in any of my slides from that district; the mineral I mistook being probably felspar.

[2] Report Cornwall and Devon, p. 27.

[3] Trans. Dev. Assoc. vol. xxi. p. 469.

[4] Q.J.G.S., vol. xl. p. 7.

[5] Brit. Assoc. Rep. 1891.

[6] Trans. Dev. Assoc. vol. xi. p. 322.

[7] Appendix, slide 40.

I have specimens of felspar veins or infiltrations, from Start Farm, Lannacombe Mill, West Prawle, Rickham Sands, Starrall Bottom, South Down, and Bolbury Down, thus covering the area of the mica and quartz schists nearly from sea to sea. This felspar constitutes an important, even though it be a minor feature, in the petrology of the district.

Veins of felspar, quartz, and chlorite, in association, are not, I believe, common in the Devonian sedimentary rocks: the only locality in which they occur to my own knowledge being on the raised beach platform at Compass Cove outside Dartmouth Harbour, where Devonian sedimentary rocks are in close proximity to diabases both north and south.[1] Here quartz is the predominant mineral, with felspar sparingly intermingled and chlorite still more so.

Sir Henry de la Beche described the Eddystone reef as "a variety of gneiss similar to some occurring near the Prawle Point."[2] Had the veteran geologist written 'West Prawle' or 'East Prawle,' instead of 'the Prawle Point,' there would be little difficulty in the case, as whereas the Prawle Point is in the area of the Green Rocks, the two villages of that name are on the mica-schists. The Eddystone gneiss has many points in common with the felspathic mica-schists, but very few with the Green Rocks. For instance in a slide of mica-schist from near the Bolt Signal Station we find quartz with minute bubbles, white mica, felspar, chlorite, and garnet. All these minerals reappear in the Eddystone gneiss, with brown mica in addition. All slaty structure has completely vanished, but I scarcely think the Eddystone is more in advance of the Bolt, than the Bolt is ahead of the Start, and the Start of the Devonian micaceous sandstones of Beesands.

Besides the points of general resemblance between the gneiss of the Eddystone and the mica-schist of the Bolt district, it is interesting to note that felspar-quartz veins with chlorite also occur in the Eddystone rock, and were described by Mr. Tawney in 1881.[3]

Two authors have noticed the green decomposition-products of the South Devon greenstones. In 1888 Mr. A. Somervail incidentally observed that both the diabases and green schists were charged with epidotic and chloritic minerals.[4] In 1889 Miss C. A. Raisin demurring to this statement, so far as it affected the diabases, said, that in them she had found chlorite to be "generally rare and often absent;"[5] but records viridite in two specimens, and a variety of palagonite in a third. Further, the authoress distinguishes between the "well-defined" chlorite of the southern rocks and the green mineral of the diabases.

After a careful examination of 28 slides of the schists and diabases I find that 14 out of 16 of the latter contain, in greater or less quantity a pale-green mineral which seems to correspond with Professor Rosenbusch's description of chlorite.

[1] Since the above was in type I have been indebted to Mr. Ussher for excellent specimens from the neighbourhood of Bantham.

[2] Rep. Dev. and Cornwall, p. 32. [3] Trans. Dev. Assoc. vol. xiii. p. 172.

[4] Trans. Devon. Assoc. 1888, p. 224. A rock from Redlap, in Mr. Somervail's collection, is crowded with macroscopic crystals of typical epidote, associated with a little chlorite. [5] Geol. Mag. 1889, p. 266.

The two remaining diabases are schistose; one, as described by Mr. Harker, contains a chlorite which polarizes in higher tints than usual, and is associated with hornblende.[1] The other, from between Modbury and Aveton Giffard, contains an apple-green chlorite, lying between the laminæ of the rock, strongly pleochroic, which sometimes polarizes the typical blue; sometimes yellow, and sometimes blue with yellow streaks. I have not myself come across any glassy rock or any mineral corresponding with the descriptions of palagonite.

The greater part of the chlorite in the Green Rocks is the strongly dichroic variety, and both in the green rocks and diabases is often associated with hornblende. Ordinary chlorite occurs in the quartz-schists at the Start and Bolt, in some of the felspathic veins, and occasionally in the green schists with the more decidedly dichroic variety. In the diabases, and in the metamorphic rocks and veins, the pale chlorite seems connected with decomposition by chemical action, while the deeper coloured, dichroic and more brightly polarizing variety seems to be closely associated with hornblende, and dynamic alteration.

I must confess to finding the chlorites very difficult. Much of this mineral in one of the diabases, when examined first by lamp light, appeared so completely isotropic that I mistook it for a glass. Even by daylight this chlorite often seems more colourless than usual, more free from any trace of dichroism, and polarizes with tints of the faintest. On the other hand the chlorite of the Schistose Diabases and of the Green Rocks often tends to the other extreme, being deeply coloured, highly dichroic, and polarizes in comparatively bright colours. On referring to those authors who have mentioned the chlorites of South Devon, their descriptions will be seen to be often qualified or guarded, *e.g.* Prof. Bonney—"rather a chlorite than a mica," Q.J.G.S. vol. xl. p. 14, and, "a species of the chlorite group," p. 16. Miss Raisin—"generally dichroic changing from a feeble brownish tint to a deep green colour, Q.J.G.S. vol. xliii. p. 719. and "possibly in part at least prochlorite of Dana, p. 720. Mr. Harker—"apparently one of the ripidolite group," *Appendix, slide No.* 40, and in a letter referring to one of the Devonian Quartz-felspar-chlorite veins, "no doubt one of the chlorite minerals, but I could not with confidence say more." Mr. Tawney, on a felspar vein in the Eddystone gneiss—"vermiform collections of clinochlore" Trans. Devon Assoc. vol. xiii. p. 172.

In describing the schist from the Start Point, Miss Raisin records the following facts, the correct interpretation of which is of crucial importance:—"In all these slides I was on the look out for evidence of secondary cleavage-foliation, and I could trace in all the beginnings of such a structure." In my own investigation of the grits and schists, specimens as free as possible from crumpling have invariably been sought for comparison; and as in these rocks the absence of crumpling proves the absence of lateral pressure, stratification-foliation and cleavage-foliation, if present, are likely to be coincident.

[1] Appendix, slide 33.

In the slide described by Mr. Harker[1] it will be observed that the little cracks cemented by opaque iron-ores maintain a rough parallelism, agreeing in direction with the scales of mica. The hand-specimen shows also the general parallelism of the quartz-laminæ. This rock is clearly a modified micaceous grit-band in which all the minerals, iron, quartz, and mica, tend to lie in planes parallel with the original bedding of the band.

Now if we turn to an equally uncrumpled Devonian micaceous grit, we find a rock macroscopically extremely like the quartz-schist, with well-defined lines of sedimentation in which the iron-ores and minute flakes of mica as seen in the microscope take a decided linear arrangement; the whole rock being also micaceous. Sometimes these Devonian grits, through pressure, crack and gape in the planes of stratification, sometimes obliquely across these planes; such cracks being occasionally filled by quartz, without the grit itself being affected by solution. Now if these grit bands with their already existing iron- and mica-lines were exposed to sufficient heat and pressure for their constituent quartz-grains to be dissolved, the mica-flakes distributed through the rock would necessarily float out into the quartz, and the two together would fill and cement cracks dependent in direction on the direction of pressure; so that these new cracks would (in the majority of cases) not be parallel with the original stratification-foliation of the grit. This simple process seems to be indicated in the less altered quartz-schists of the Start, in which to judge by analogy with the Devonian grits the black lines of iron and mica usually correspond with the original sedimentary lines, while the quartzose laminæ are in nearly all cases of subsequent date.

It may be noted that among the minerals accompanying and determining foliation in the mica- and quartz-schists of South Devon are iron, mica, quartz, and felspar, and in the green rocks felspar, hornblende, and chlorite. In the Devonian Volcanics the laminæ of the schistose rocks are often determined by hornblende and chlorite, but I have not noticed any distinct alineation of felspar, as distinguished from veins.

EXPLANATION OF PLATE VIII.

FIG. 1.—Red Mica Schist, north of Start Signal House. (24) Magnified 13 diameters. Original streaks of red and black iron ores traverse the slide in places obliquely from top to bottom. These are intimately associated with mica, which lies between them in wavy lines. These more ancient minerals are invaded by quartz, which traverses the slide from side to side. The mica-flakes disengaged by the quartz tend to align themselves flake by flake in the general direction of the quartz. In this rock, though a mica schist, the planes of schistosity are indicated by iron and quartz, the direction of the mica being uncertain.

FIG. 2.—Chlorite-schist, Rickham Sands, Salcombe. Magnified 31 diameters. Secondary felspar-granules with belonites in a chloritic matrix. The schistose character of the rock is determined by the general alignment of the felspar-granules.

(*To be continued in our next Number.*)

[3] Appendix, slide No. 8.

II.—Notes on the Coniston Limestone Series.

By J. G. Goodchild, F.G.S.,

H.M. Geological Survey.

Communicated by permission of the Director for Great Britain.

MR. MARR'S valuable communication on the Coniston Limestone Series, which appeared in the March Number of this Magazine, is sure to be welcomed as an excellent summary of the geology of this interesting group of rocks. It contains a digest of most of the published work relating to the Coniston Limestone Series, and also many new facts and arguments drawn from Mr. Marr's own observations in the field.

The stratigraphy of some of the areas referred to in the paper presents very considerable difficulties, and therefore requires very detailed observations before any definite conclusion can be drawn from the facts. This is perhaps the reason why so many observers have differed in their interpretation of the evidence. In the areas adjoining the Pennine-Craven Fault especially the geology is so complicated that many square miles of country might be described as consisting of a gigantic fault-brecia, whose constituents appear to defy any attempt at identification. The officers of the Geological Survey had to go over a large part of this faulted area again and again, long after every available piece of evidence within the area itself appeared to be exhausted. All this requires much time. But one result of such repeated revisions of the more difficult parts is that those who have gone so many times over the ground gain obvious advantages over those who happen to be less fortunate in such matters.

Under the circumstances, therefore, Mr. Marr will hardly be unprepared to find that, in minor points of detail relating to the rocks under notice, some of his predecessors have arrived at conclusions different from his own. That is so in the present case; and I avail myself of the Director's permission to call Mr. Marr's attention to one or two such, in the hope that the corrections may be of service to him in his future work over the same ground.

The Bala Rocks of the Cross Fell Inlier.—The highest members of this series have already been adequately described by Messrs. Marr and Nicholson (Q.J.G.S. vol. xlvii.), so that there is no need to discuss any special point in connexion with them here. For field purposes it suffices to separate these rocks into a (1) lower series consisting of calcareous shales, and containing a fauna proper to pelitic rocks of this age. (2) An upper, mainly calcareous series, with the fauna such as might be expected to occur in the clearer waters where limestone was in process of formation. Any local change from argillaceous to calcareous is, as might be expected, accompanied by a corresponding change in the fossils. The lower series of shales graduates upward into the calcareous series. The limestone of Keisley belongs, I believe, to a higher part of this calcareous series than has been left by pre-Silurian denudation elsewhere in the area under notice. It is faulted in all round.

Below the Upper Shale-and-limestone series, which represents the Coniston Limestone Series of the areas to the west, there rises a thick mass of rhyolitic tuffs, which form the Pikes of Dufton, Knock, the north end of Rake Brow, Melmerby, and several other smaller eminences in the district. They are quite conformable to the overlying shales. Their base is not clearly seen anywhere here; but quite enough of them is exposed to show that they are not less than 1100 feet in thickness. This, there is reason to believe, is not far from their full vertical extent. Except that these rocks are (as I believe) of pyroclastic origin everywhere in this area, they very closely resemble the rhyolitic series that underlies the Coniston Limestone west of Shap, and there cannot be much doubt about their actual contemporaneity.

With the general distribution of these in the Cross Fell area we are not at present concerned. But their presence in the neighbourhood of Roman Fell has been misinterpreted. Considering the complicated nature of the geology there, this can hardly be wondered at. The facts as they appear to me are of the following kind:—Between the village of Helton and the flanks of Roman Fell, Coniston Shales of the ordinary type are seen at many places from Helton Beck southward. They are cut off on the south-west by the Outer Pennine Fault, which brings the Bunter (or St. Bees) Sandstones and Shales directly against them. The Coniston Shales dip towards the north-west at rather high angles. So that as their outcrops are traced towards the south-east, their base is reached about five hundred yards to the south-east of Helton Beck. Then rises from beneath them here, as elsewhere in this part of England, a mass of rhyolitic tuffs of exactly the same nature as those of Dufton Pike, Knock Pike, etc. These form the small hill known as the Seat. The Outer Pennine Fault runs close alongside this hill, and cuts off these tuffs on the south-west, bringing the St. Bees Sandstones into direct contact with them for a distance of several hundred yards, as may be seen in the course of Helton Sike.

On the eastern side of the Seat ranges one of the Middle Pennine Faults, which here brings up against the rhyolite tuffs a narrow strip of the alternations of submarine tuffs and argillites, which represent the seaward equivalents of the Borradale Series, and which I have named the Milburn Rocks. These in turn are cut off by another fault ranging nearer to Roman Fell. The effect of this fault is to let in a strip of volcanic rocks of yet a different type. It is these with which we are specially concerned at present. Under the name of the Roman Fell Volcanic Series, specimens collected and fully labelled by myself were for several years from 1876 onward on exhibition in the Rock Collection at the Museum of Practical Geology. I have reason to know that Mr. Marr has long been acquainted with the specimens in question; though I understand from him that he has not actually seen them *in situ*.

On the ground, these Helton Moor Volcanic Rocks may be readily found by any one used to field geology, if he will cross the Moor from the Smelt Mill at Helton along a direct line connecting that

building with a point a little to the north of the summit of Roman Fell. Both points can easily be kept in view. He will cross (1) the Bunter Sandstones and Shales; (2) the Outer Pennine Fault, which runs north-westerly along the upper side of Helton Sike; (3) the Coniston Shales, seen in several places on the Moor; (4) the Rhyolite tuffs (Dufton Pike beds) of the Seat; (5) a narrow strip of the Milburn Rocks faulted in by the Middle Pennine Fault; (6) the Helton Moor Volcanic Series, and also an interesting dyke of microgranite, containing large rhombs of muscovite, like the Dufton "Granite." These are best exposed along the 1250 feet contour. When this is reached, the observer will see the rocks best by following the same elevation southward. Their outcrop extends over about seven hundred yards. These volcanic rocks will be seen to have a fairly-steady, and high, northerly dip, so that lower beds rise to the surface as the rocks are traced southwards. They consist of alternations of ashy mudstones, ashy grits, tuffs, and a few beds of lava, which occur chiefly at the north end of the exposure, and are, apparently, of an andesitic type. One or two beds of lava are markedly porphyritic, and recall the beds of Rake Brow, Melmerby (whose volcanic origin was demonstrated by the Survey in 1876).

As the beds are followed towards the south, their base is reached, and the volcanic rocks are seen to graduate downward into a group of very calcareous and partly ashy-looking shales. I have published references to this important piece of evidence on many previous occasions, and have, in addition, had the pleasure of conducting many fellow geologists over the rocks in question during the last fifteen or more years.

We are indebted to Messrs Marr and Nicholson for very valuable lists of fossils from these beds. On account of the occurrence in them of *Trematis corona*, the authors just named have called these beds the Corona Beds. The name seems to me to be not altogether a good one, and for the following reasons:—*Trematis corona* occurs in the Coniston Shales in several localities. Below these shales come the rhyolite tuffs, which are hardly less than 1100 feet in thickness. Immediately beneath them the exact sequence is not known; but I do not think that any great thickness of rock (if any at all) is missing. Then follows the series of volcanic rocks of Helton Moor, just referred to, which are certainly not less than 1000 feet in thickness. (I more than suspect that these Helton Moor volcanic rocks are the equivalents in time of those I named the Rake Brow Series, from the place of that name east of Melmerby. Certainly some beds very much like the Helton Moor beds occur at the base of the series at Rake Brow; just as rhyolitic tuffs like those of Knock come on above them.) Below the Helton Moor volcanic rocks is the Lower *Trematis corona* bed, that is to say, the fossiliferous shales mentioned above.

I think if Mr. Marr had acted upon the friendly hint I gave him in a letter some time back, he would have found that there are two *Corona* beds here, and that these are separated by a considerable thickness of other rocks, as Professor Lapworth informs me is the

case in the Girvan area. Certainly the two *Corona* beds do not come together, as Mr. Marr's sections indicate.

As for the relation of these Helton Moor beds to the interesting set of strata discovered by Messrs. Marr and Nicholson at Drygill, I fail to see any reason why they may not be contemporaneous. It is now several years since I expressed the opinion that the higher Ordovicians of Cumberland were transgressive across successively lower beds of the same system as they trend towards the north; and illustrated that view by a diagram (Proc. Geol. Assoc. vol. ix. No. 7, fig. 1). This diagram was intended to express what I believed to be the facts in not only the Lake District, but also in the Cross Fell area.

It may be convenient to give here a general summary of the succession of these older rocks as developed in the area at the foot of the Cross Fell Escarpment:—

	Approximate thickness in feet.
Silurian Rocks.	
Unconformity.	
Coniston Limestone Series	+ 900
Dufton and Yarlside Rhyolitic rocks	+1100
Strata missing, probably not of great thickness.	
Helton Moor (? and Rake Brow) Volcanic Series	+1000
Helton Moor Shales...	+ 500
Break of unknown extent, probably coinciding with an unconformity, which increases in extent going northwards.	
Milburn Rocks... .. .. not less than	6500
Skiddaw Slates, of great thickness ... ... not less than	8000

The Craven Area.—I shall not attempt, on the present occasion, to deal with the many difficult problems suggested by the Bala rocks of this area. All the older Palæozoic rocks of Craven were mapped in great detail by Prof. Hughes, and were described by him, many years ago, and both Mr. Marr and I owe much of our knowledge of them to his earlier work. I may, however, state that they seem to me to be much more complex, and also very much thicker, than has generally been supposed. It is a long way from Roman Fell to Wharfe: otherwise I should be disposed to think that the beds in Craven that Mr. Marr has taken to be on the horizon of the Coniston Limestone are much lower down in the series, and that the true Coniston Limestone (here much thicker than in the typical area) was locally denuded (together with a great thickness of some rocks older in the series) before the Silurians were laid down; so that it is only left here and there. I had the advantage of going over all the details of that part of Prof. Hughes' work under his own guidance, and with the additional advantage of the long experience of Mr. Aveline. This was between 1874 and 1876. Later still, my colleague, Mr. Gunn, carefully checked all our previous work, under the superintendence of Mr. Howell, the Director for Great Britain. I think I may now venture to state on behalf of all my colleagues concerned, that not one of us is at all disposed to entertain for a moment the idea that the Bala rocks of Craven are separated from the Ingleton Green Slate Series by a fault, as Mr. Marr has shown in his section. On the contrary, the Bala calcareous-and-ashy series appears to us

to graduate downward by interbedding into the great series of barren greywackes and argillites, which we call the Ingleton Green Slate Series; so that the upper and the lower sections are almost inseparable.

Like Mr. Marr, we were all of us at first very much struck by the remarkable difference in lithological character between these sedimentary rocks of Ingleton and the tuffs and lavas of the Lake District. Mr. Aveline, whose authority in such matters no geologist will question, repeatedly expressed the opinion, as far back as 1874, that he could see no difference between this Ingleton Green Slate Series and the *Longmyndians*. Nor can I. Nor is there any difference between them and the rocks of the Scottish Central Highlands, except that the latter rocks have undergone plutonic metamorphism, which the Craven rocks have not. But then, on the other hand, one great series of greywackes is very much like another, and much of the Coniston Grit, as Mr. Marr knows better than most men, is hardly distinguishable in lithological character from much of the Ingleton rocks.

I see no difficulty in regarding the Ingleton rocks as old sediments like the coarser part of the Milburn Rocks, into which marine currents have swept the detritus from the contemporaneous denudation of the maritime volcanoes to the north.

In conclusion there is one point that Mr. Marr has not dwelt upon at as great length as his knowledge of the subject would have warranted. It is this:—the most widely-spread types of Ordovician rocks in the north-west of England must not be looked for in the Lake District proper, but in the areas where deeper-water conditions obtained outside of it. The persistent types are to be found, not in Mid-Cumberland, but in Craven, and within the Cross Fell Inlier.

III—On the British Earthquakes of 1891.

By Charles Davison, M.A.;
Mathematical Master at King Edward's High School, Birmingham.

COMPARED with that of the year before, the British seismic record for 1891 is somewhat meagre. It includes a slight earthquake felt in North Cornwall on March 26, several at Invergarry, Ardochy, and Loch-hourn Head in Inverness-shire; and one other, believed to be that of an earthquake, at Bournemouth on October 25. The remarkable series of earthquakes felt between November 15 and December 14, 1890, in the district round Inverness seems to have ended at the latter date; for Mr. J. Birnie, a very careful observer residing at Balnafettack, informs me that he has felt none during the past year; and his evidence is especially valuable, since the epicentra of nearly all the shocks must have been close to that village.

N. Cornwall Earthquake: March 26, 1891.

Time of occurrence, about 11 h. 30 m.; Intensity, IV.; Epicentrum, lat. 50° 40′ N.; long. 4° 36¾′ W., *i.e.* 4 miles N. 35° E. of Camelford.

Disturbed Area.—I have received seventeen accounts of this earthquake from sixteen different places. On the accompanying map these places are represented by small discs; a cross drawn through the discs indicating that the usual earthquake-sound was also heard. At one place (Otterham), so far as I know, the sound only was noticed. At three other places near the boundary of the disturbed area, the shock does not seem to have been felt, though special inquiries were made by residents in them. These places are Egloskerry, St. Kew and Trewen; and they are represented on the map by small circles. At Laneast, Port Gavern, St. Breward, St. Gennys and St. Tudy, I have been informed that the shock was not felt, but I am not certain that this was the experience of more than one person at each of these places.

The boundary of the disturbed area, as drawn upon the map, corresponds to an isoseismal line of intensity somewhat less than IV. according to the Rossi-Forel scale. The area inclosed by it is 17 miles long, 13 miles broad, and contains about 170 square miles. Its centre is 4 miles N. 35° E. of Camelford; and this point probably coincides very nearly with the position of the epicentrum. The longer axis of the disturbed area is directed approximately north and south. About one-third of the boundary-line, it will be noticed, lies outside the land; and its exact course in this part must of course be somewhat uncertain. The observations from Tintagel and Boscastle show, however, that its position cannot differ very greatly from that assigned to it on the map.

Nature of the Shock.—The following accounts give the most detailed information I have received:

Bolverton.—Two distinct shocks felt, separated by an interval of two or three seconds.

Boscastle.—(1) Two shocks, the first rather more intense than the second which followed after a lapse of perhaps ten seconds, a decided tremulous motion being felt both during and after the two shocks. (2) Two shocks, the second shorter and fainter than the first, and felt a second or two after it.

Jacobstow.—Two shocks, the interval between them two or three seconds.

Michaelstow.—Two shocks felt, the first very slightly, the second very decidedly, and almost instantaneously after the first.

New Mill, Poundstock.—Two shocks, a slight tremulous motion being felt before and after both of them, the first shock more intense than the second.

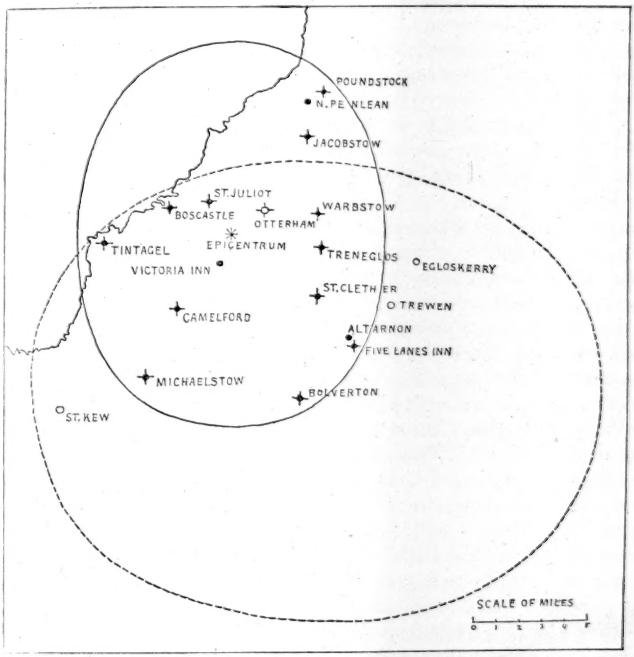
St. Clether.—Two shocks, consisting of a trembling like that caused by a sudden violent rush of wind against the house; each shock lasted about ten seconds, they were approximately equal in intensity, and were separated by an interval of a few seconds.

St. Juliot.—Two shocks, consisting of rapid and continuous vibrations, slightly increasing in intensity about the middle. The first and strongest shock lasted for eight or nine seconds, the second for five seconds, the interval between them being three seconds.

Tintagel.—Two shocks felt.

Treneglos.—Two shocks felt, separated by an interval of about fifteen seconds, no vertical motion perceptible.

Intensity.—The intensity of the shock was at IV. at the following places: Altarnon, Boscastle, Camelford, Poundstock, St. Clether, St. Juliot, Tintagel, Treneglos, Victoria Inn (near Davidstow) and Warbstow; and less than IV. at Bolverton, Five Lanes Inn (near Altarnon) and Michaelstow.



Sound-phenomena.—With three exceptions (Altarnon, N. Penlean, and Victoria Inn) earthquake-sounds are recorded at all the places of observation, and the omission at these places is probably accidental. The sounds were heard at all the places which determine the boundary of the disturbed area. They were also, I am informed, heard two miles to the east of Bolverton, so that it is possible that the sound-area may have slightly overlapped the disturbed area towards the south-east. Otherwise the two areas seem to have coincided approximately.

The sound is said to have preceded the shock at Michaelstow, to have accompanied it at Bolverton, Boscastle, Camelford, Jacobstow, St. Clether, St. Juliot and Treneglos, and to have preceded, accompanied and followed it at Poundstock.

At Boscastle, Dr. Wade informs me that the sound resembled that of a ponderous waggon going over a vacuum. It was heard while the shocks lasted and during the whole of the interval of about ten

seconds between them; the intensity being nearly uniform throughout, but greatest when the vibrations were felt, and slightly less during the interval between them. Another observer at the same place compares the sound to that made by an unusually heavy waggon going over a bridge, but it was not heard by him during the whole of the interval between the shocks. "There was silence," he remarks, "for a second or two between the rumbles; the latter went off as the sound of a heavy ground sea after breaking on the shore; it was shorter and fainter than the first." At every other place the sound ceased for a greater or less time between the shocks. At Michaelstow a loud rumbling sound, resembling the passing of a very heavy vehicle over a hard road, preceded each shock, growing rapidly in volume and dying quickly away. At St. Juliot, again, the sound accompanied each shock, varying only slightly in intensity, but greatest at the moments when the vibrations were felt.

Origin of the Shocks.—The first point to be determined is the origin of the double shock, and of this three explanations may be given. (1) The seismic focus may have consisted of two detached portions, or have contained two regions of maximum initial intensity; (2) there may have been a repetition of the originating impulse at one and the same spot; or (3) a repetition of the originating impulse at another and different spot; the focus, or line, joining the foci, being in any case directed north and south.

If the first explanation were the correct one, the interval between the two shocks would have been greatest in the continuation of the line joining the foci, *i.e.* in the north and south parts of the disturbed area, and least in the east and west parts. Without placing too great a reliance on the estimates of this interval given above, it is clear that they offer little support to this theory. At Michaelstow, indeed, the second shock was said to follow "almost instantaneously" after the first, but the interval was yet long enough to allow the rumbling sound to cease and to be heard again before the second shock; and Michaelstow is not far from the southern boundary of the disturbed area. The second explanation is also out of the question, on account of the different relative intensity of the shocks at different places.

There remains the third theory, and the evidence, such as it is, seems clearly in its favour. At all parts of the disturbed area there would be a perceptible interval between the shocks, provided the interval between the initial impulses was not equal to the time required for the earth-wave from a further and earlier focus to overtake that from a nearer and later one. The distance between the foci will, however, account to some extent for the variations in the estimates of the interval between the shocks.

More important evidence is furnished by the different relative intensity of the two shocks at different places. We have seen that the first shock was the stronger at Boscastle, Poundstock, and St. Juliot, all lying to the north of the epicentrum; the second decidedly the stronger at Michaelstow, near the southern end of the disturbed

area; while at St. Clether, which is near the eastern boundary, the intensities of the two shocks were approximately equal.

I conclude, then, the shocks were really distinct, the focus of the second being to the south of that of the first; and it is obvious that, if this were the case, the interval between the two shocks should have been least at the southern end of the disturbed area, an inference which agrees with the estimate already quoted of the apparently short interval at Michaelstow.

The position indicated on the map as that of the epicentrum probably corresponds closely with the superficial position of the centre of intensity of the whole seismic focus. The distance between the two foci may have been as much as two or three miles, but Dr. Wade's interesting observation at Boscastle of the sound being heard during the whole interval between the shocks seems to show that the foci were not completely isolated. If the shocks were due to fault-slipping,[1] we may infer, from the form of the disturbed area and from the facts summarised in the preceding paragraphs, that the direction of the fault must be north and south. Boscastle, again, is the only known place where the sound was heard continuously between the two shocks, and, as mentioned above, the sound-area may have overlapped the disturbed area along its south-east margin. These facts seem to indicate, but not with certainty, (1) that Boscastle must be near the spot where the normal to the seismic focus meets the surface of the earth, and (2) that the upper margin of the focus from which the sound-vibrations proceeded lay to the east or south-east of the epicentrum; *i.e.* that the fault must hade to the westward. Lastly, if this conclusion be correct, the line in which the fault intersects the surface must pass to the east of the epicentrum, and at a distance from it probably not much greater than one or two miles.[2]

Summing up on the supposition that the earthquake was fault-formed: the first shock was probably caused by a slip within a small area about a mile north of the point marked as the epicentrum; but the slip continued southward for about two miles, though so slight in extent that only earthquake-sounds were produced, an interval of perhaps five or more seconds being necessary for the slipping over this distance to take place; at the southern end the slip being again great enough within a small area to produce a shock of approximately the same intensity as the first.

It is interesting to notice the relation between this earthquake and that felt in East Cornwall on October 7, 1889. The boundary of the disturbed area of the latter is indicated on the map by a dotted line. Its epicentrum lay about $2\frac{3}{4}$ miles S.W. of Altarnon, and the

[1] There is no fault marked in the Survey Map of the district with which the earthquake can be connected.

[2] The fact that no shock was noticed at Otterham is in favour of the position above assigned to the fault; for, if the earthquake were due to fault-slipping, the earth-waves in the rock-masses on either side of the fault would start in opposite phases of vibration and might possibly interfere to a very great extent along the line of fault. (See a paper "On the Existence of Undisturbed Spots in Earthquake-shaken Areas," GEOL. MAG. Dec. III. Vol. III. 1886, p. 157).

longer axis of the disturbed area ran east and west, *i.e.* parallel to the direction of folding in the district. The earthquake of 1889 was therefore a *longitudinal* earthquake, and that of 1891 a *transverse* earthquake.

Authorities.—The published accounts of this earthquake are very scanty, and for all that is of value in the preceding description I am indebted to the kindness of the following ladies and gentlemen who courteously replied to my inquiries either addressed to them or printed in two or three of the local newspapers: Bolverton, Rev. S. G. Gregory; Boscastle, Dr. A. Wade and Mr. J. Brown; Camelford, Mr. E. Rogers; Egloskerry, Rev. W. S. Sloane-Evans; Five Lanes Inn, Mr. A. Chapman; Jacobstow, Rev. F. T. Batchelor; Michaelstow, Rev. W. H. Gillett; Otterham, Rev. E. H. Archer-Shepherd; Poundstock, Mr. J. D. Graf; St. Clether, Rev. F. Partridge; St. Juliot, Mr. W. H. Sanders; St. Kew, Mr. W. Hitchin; Tintagel, Miss B. F. Cooke; Treneglos, Mr. J. C. Chapman; Victoria Inn, Mrs. Greenwood; Warbstow, Mr. W. Petherick.

Earthquakes in Inverness-shire.

For the following list of shocks I am indebted to the valuable observations of Mr. John Grant of Invergarry; Mr. Murdoch Matheson, who has removed from Feddan to Ardochy; and Mr. A. Campbell, of Loch-hourn Head. Ardochy lies on the north side of Loch Garry about a mile from its west end. At Glen Quoich and Glen Kingie, earthquake-shocks are occasionally felt, though none has been observed at either place during the past year by Mr. D. Grant and Mr. A. G. Foster, both of whom are doing useful work in recording the occurrence of earthquakes felt by them. It is worthy of notice that all the six places here mentioned lie close to a line joining Invergarry and Lochhourn Head. Every one of the seventeen or eighteen shocks mentioned below was felt by more than one observer.

Feb. 24. 22h. 55m., Invergarry, resembling the sound of a carriage.
„ 23h. 20m., Loch-hourn Head, a rumbling noise with a slight movement, lasting fully ten seconds. This shock may be the same as the preceding, but the distance between Invergarry and Loch-hourn Head is 23 miles, and no shock is recorded about this time at either Ardochy, Glen Quoich or Glen Kingie.
Feb. 25. 1h. 15m., Invergarry, like a heavy carriage passing.
Mar. 1. 15h. 15m., Ardochy, a shock lasting eight seconds.
„ 21h. 25m., Ardochy, a stronger shock than the preceding, lasting four seconds, and preceded by a rumbling noise.
Mar. 2. 22h. 15m., Invergarry, like a heavy carriage passing.
Apr. 24. 14h. 30m., Ardochy, a shock of intensity IV., followed by a noise resembling thunder.
Aug. 27. 4h. 30m. and 6h., Invergarry, like a heavy carriage passing.
Aug. 30. 16h. 15m., Invergarry, the same.
Nov. 16. 10h. 25m., 14h. 15m., and 20h. 45m., Ardochy, slight shocks without any accompanying noise.

Dec. 6. 9h. 55m., Invergarry, like a heavy carriage passing.
Dec. 26. 1h. 20m., Loch-hourn Head, a very slight noise and trembling of the floor on which the observer was standing.
Dec. 28. 20h. 20m. and 21h. 24m., Invergarry, like a heavy carriage passing.
Dec. 30. 9h. 45m., Invergarry, like a heavy train crossing a bridge.

Doubtful Earthquake.

The following shock is entered under this heading, having been noticed by only one observer; but, judging from the careful description, I believe there can be little doubt as to its seismic nature.

October 25, 1891, *about* 16*h.*, *Bournemouth.*—A notice of this shock by Mr. Henry Cecil appeared in *Nature* (vol. 44, p. 614), and this, together with a more detailed description which Mr. Cecil kindly sent me, is the source of the following account. A dull thud was heard, as of a heavy fall underground, and instantly afterwards (all but simultaneously with it) a single momentary shock without any preceding or following tremor. The observer's eyes were directed at the time on a plant resting on the table beside him, and, when the shock occurred, the long, pendant, lightly-poised leaves of the plant were violently agitated, waving up and down through a large arc for several seconds. The movement of the ground must have been vertical, and Mr. Cecil remarks that his impression was that it was first upward and then downward. It being Sunday afternoon, almost all traffic was suspended, but, shortly after the shock, a heavy carriage passed along the adjoining road without producing any perceptible movement in the plant. Mr. Cecil informs me that he has felt several slight shocks at Bournemouth, and he believes that in the present case the sound and shock were of seismic origin.

IV.—Was the Boulder-clay formed Beneath the Ice?

By G. W. Bulman, M.A., B.Sc.;
Corbridge-on-Tyne.

GEOLOGICAL opinion is still divided on the subject of the formation of the Boulder-clay. By some it is held to be the sole work of the ice, and accumulated beneath it; by others it is looked upon as a marine deposit, though deriving its materials from the grinding action of the ice-sheet or glacier. Some geologists, again, hold that the "Till"—as distinct from the Boulder-clay—was formed beneath the ice, but that the latter is a marine deposit laid down in glacial seas.

Neither the first nor the third of these views can as yet be said to have received the stamp of geological certainty, for it has not been conclusively shown that any such deposit is being formed beneath the ice at the present day; nor can it be inferred from what we know of the properties of ice in the form of glacier or ice-sheet that such a deposit *ought* to be the result of its action.

Thus no deposit analogous to Boulder-clay is found in Switzerland

where the ice has retreated within recent times. With regard to this point Prof. Bonney writes as follows:

"The Glaciers of the Swiss and Savoy Alps have been retreating for several years, hence, if anything like ground moraine existed, this would be a very favourable time for observing it. In no case have I been able to find signs of any deposit resembling Till or Boulder-clay; the detrital matter which is scattered, generally sparsely, over the slope left bare by the retreating glacier has fallen from its surface, like ordinary terminal moraine. Further, by availing myself of crevasses, etc., I have made my way occasionally for some little distance beneath the ice. Nothing has been seen but bare rock, with now and then a film of mud or a passing stone. In short, the result of an experience of some years has convinced me, that if anything like the Till or Ground Moraine of recent glacialists exists in the Alps, it is a very local and exceptional phenomenon."[1]

And so we find a tendency among those who believe the Boulder-clay was accumulated beneath the ice, to push back the question of its origin from the comparatively known regions of Alpine glaciers to the unknown sub-glacial area of Central Greenland.

Thus Sir A. Geikie refers to what *must* be taking place beneath a vast continental ice-sheet:

(1) "In such comparatively small and narrow ice-sheets as the present glaciers of Switzerland, the rock bottom on which the ice moves is usually, as far as can be examined, swept clean by the trickle or rush of water over it from the melting ice. But when the ice does not flow in a mere big drain (which after all, the largest Alpine valley really is), but overspreads a wide area of uneven ground, there cannot fail to be a great accumulation of rubbish here and there underneath it. The sheet of ice that once filled the broad central plain of Switzerland between the Alps and the Jura certainly pushed a vast deal of mud, sand, and stones over the floor of the valley. The material is known to Swiss geologists as the *Moraine profonde* or Grundmorane (=Boulder-clay, Till, or bottom-moraine)" (Text-Book of Geology, p. 411).

(2) "We know as yet very little regarding its (the Grundmorane) formation in Greenland. Most of our knowledge regarding it is derived from a study of the Till or Boulder-clay in more southern latitudes, which is believed to represent the bottom moraine of an ancient ice-sheet (*ibid.* p. 417).

The weakness of the argument is further illustrated by the following passage from the Physical Geology of Prof. Green, in which I have italicised certain parts:

"We have already seen that under such a sheet [continental ice-sheet] there is *probably* formed an accumulation of clay and stones known as Moraine profonde or Grund-moräne, and Till resembles exactly what we *picture* to ourselves that this deposit *must* be like. There would be weight enough to give rise to the intense toughness and the close and irregular packing of the stones, and the

[1] Geol. Mag. Vol. XIII. p. 197.

scratching and polishing would be produced as the mass was moved hither and thither by the flow of the ice. . . . . .

Though the existence of the Moraine profonde is *to a certain extent hypothetical*, the *probability* that such an accumulation is formed beneath large ice-sheets is so great, and its character, *if it exists, must be so exactly* that of Till, that nearly all geologists are now agreed to look upon the latter as having been formed by the grinding and wearing away by an ice-sheet of the ground on which it rested" (p. 264).

But as far as evidence from Greenland is available it throws as little light on the origin of Boulder-clay as the Swiss valleys. Thus Nordenskiöld describes an area lately left by the ice as containing no moraines, and in his description makes no mention of Boulder-clay: "We passed, in fact, over ground that had but lately been abandoned by the inland ice. . . . . Everywhere occur rounded, but seldom scratched, hills of gneiss with erratic blocks in the most unstable positions of equilibrium, separated by valleys with small mountain-lakes and scratched rock surfaces. On the other hand, no real moraines were discoverable. These, indeed, seem to be commonly absent in Scandinavia; and are, generally speaking, more characteristic of small glaciers than of real Inland Ice" (Arctic Voyages, Macmillan, 1879, pp. 169, 170).

And further, in describing the clay-beds of Greenland, Nordenskiöld speaks of them as formed *outside* the ice-sheet:

"The material of the clay-beds has evidently been deposited by the glacier rivers whose muddy water everywhere burst out from under the inland ice, but in general the deposits are sea formations, *i.e.* they have been deposited under the level of the sea" (Geol. Mag. Vol. IX. p. 410).

And no deposit resembling Boulder-~~clay~~ is described by him as occurring on that strip of Western Greenland not covered by the ice-sheet at present.

Similar remarks may be made with regard to those areas in America where a retreat of the ice permits an examination of the ground recently occupied by it. In the American Journal of Science, March, 1892, for example, is an interesting description of Mount St. Elias and its Glaciers. Where the ice has retreated glaciated surfaces are seen, but *no Boulder-clay*. Spread out, however, over the whole area between the ice and the sea is a mass of stony moranic matter. *Streams from the glaciers carry the finely-ground rock matter into the sea.*

The probability or otherwise of the Boulder-clay having been formed *beneath* the ice may be further considered in the light of what we know of the physics of glacier action. To meet the requirements of the geologist, the ice, in the form of glacier or ice-sheet, must accomplish—and apparently *does* accomplish—a variety of seemingly incompatible actions. It must clear the ground of superficial accumulations and grind and polish the rock below, while at the same time it must glide over loose deposits of clay and stones without disturbing them; it must round and polish certain

stones while carrying others for long distances without taking off their sharp edges; it must hold firmly embedded in its lower surface stones with which to striate and groove the rocks below, while at the same time it leaves a large number behind to form the moraine profonde.

And when we turn to the glaciers themselves we see them accomplishing a series of actions as apparently incompatible. For while the ice *flows* along its bed more or less like a river, it yet polishes and striates the rocks below, apparently by holding stones firmly grasped in its lower part; and while grinding down the rocks over which it passes to the finest powder, it has yet been observed to rise gently over loose deposits of clay and stones without disturbing them; at times it allows stones to sink into it, so that its surface moraines disappear, while at other times it works up the stones from below to the surface, and they appear again.

While the anomalies of Glacial action are so great it would be rash to assert that Boulder-clay cannot be formed beneath the ice, but the great fact of constantly running sub-glacial streams renders it improbable. These streams laden with fine rock-flour issue from the termination of glacier and ice-sheet summer and winter. How far such streams ascend the glacial valleys it is difficult to say. *Probably* they extend at least as far as the point where the névé gives place to the ice-river. Such a conclusion, at least, seems to follow from certain considerations as to the probable temperature beneath a considerable sheet of ice or snow.

Ice and snow are bad conductors of heat, and hence the lower surface of a thick ice-sheet will tend to take the temperature due to the surface of the earth—say, about 50° F. The pressure, moreover, of the superincumbent ice will tend to liquify the lower portion. Such streams running beneath the entire extent of the glacier proper would readily account for the number of rounded stones found in the Boulder-clay, as well as for intercalations of sand and gravel; but do not permit the supposition of a sufficient quantity of rock-flour being left behind to form more than such fragmentary fringes and patches as a river leaves along its course. The *greater part* of this finely ground rock matter will be carried *beyond* the limit of the ice, and if the glacial streams spread out over level plains deposits of clay of considerable extent *might* be formed on land. Most of this sediment, however, will be laid down in water. If we study the distribution of glacial mud in Switzerland at the present day we find it is mostly laid down in lakes. Many of its glacial mud-laden streams fall into lakes from which they emerge clear and pure, having deposited their sediment therein. Thus the Rhine flows into Lake Constance and the Unter Sea, the Rhone into the Lake of Geneva, the Aar into the Lakes of Brienz and of Thun, the Ticino into Lake Maggiore, the Reuss into the Lake of Lucerne.

This fine sediment would provide the clay, and if *boulders* could at times be carried in, the conditions for the formation of boulder-clay would be present in these Swiss lakes to-day. And, whatever may

be the case now, when the ice extended further and reached the lakes, small bergs would be frequently detached, and melting, allow their burden of stones to sink to the bottom of the lake amid the clay there accumulating.

Prof. Nordenskiöld, again, has described how a similar deposit is being formed in Greenland at the present day.

"At the foot of the glacier," he writes, "we often find, as in fig. 2, ponds or lakes in which is deposited a fresh-water glacial-clay, containing angular blocks of stone, scattered around by small icebergs" (Arctic Voyages, pp. 170, 171, Macmillan, 1879).

But it is when we turn to the sea that we find the most promising method of accounting for the origin of Boulder-clay in general. For it seems obvious that deposits analogous to Boulder-clay must be accumulating on a large scale in the seas off the coasts of Greenland and other arctic lands. Sub-glacial rivers, laden with the finer products of glaciation, are discharging themselves into the sea throughout the year. Icebergs, also, are constantly being formed and floated away with their loads of debris. Part of this debris no doubt will be deposited only when the bergs melt in more southern latitudes; but a part will also be laid down near the coast and among the finer sediment by the overturning of the bergs, which not unfrequently happens. And it is, perhaps, worthy of note that large quantities of glaciated stones may be carried by such bergs and deposited amid accumulations of clay taking place in latitudes far south of the limits of glaciation. A clay full of glaciated stones may thus *not necessarily* imply the glaciation of the latitude where it occurs.

The opinion that the Boulder-clay was thus formed in the sea has been expressed by eminent geologists. Thus Sir J. W. Dawson in his "Handbook of Geology for Canadian Students," expresses his opinion thus:

(1) "Under these circumstances moraines were formed on the land, and sheets of stony clay with boulders in the sea, forming what has been termed the Boulder-clay or "Till" (p. 114).

(2) "That this Boulder-clay is a sub-marine and not a sub-aerial deposit, seems to be rendered probable by the circumstance, that many of the boulders of the native sandstone are so soft that they crumble immediately when exposed to the weather and frost" (p. 154).

And Prof. Boyd-Dawkins writes: "The hypothesis . . . . . that the Boulder-clays have been formed on land is open to the objection that no similar clays have been proved to have been so formed, either in the Arctic regions, where the ice-sheet has retreated, or in the districts forsaken by the glaciers in the Alps or Pyrenees, or in any other mountain-chain. Similar deposits, however, have been met with in Davis Strait and in the North Atlantic, which have been formed by melting icebergs; and we may therefore conclude that the Boulder-clays have had a like origin" ("Early Man in Britain," pp. 116, 117).

The chief difficulty in the way of this marine origin of the Boulder-

clay is perhaps the absence of lamination, so characteristic of deposits in water. But a consideration of the facts of the case suggests a possible explanation. The two general causes of original lamination —indeed I am not acquainted with any other—are the intermittent supply of sediment, and the presence of flat plates of mica. Both of these are probably absent in the case of the deposit in question. The ice provides a *continuous* supply of rock flour, and rivers laden with it flow from the glacier and ice-sheet all the year through; and it permits the existence of no large flat plates of mica.

The general conclusion, then, to be derived from a study of glacial action seems to be that while the greater part of the rock-flour which goes to form the clay is carried beyond the limits of the ice and laid down in lakes, the sea, or on land, fringes and patches of the same may be left behind in the valleys occupied by the glacier or ice-sheet.

V.—Glacial Geology: Old and New.

By T. Mellard Reade, C.E., F.G.S., F.R.I.B.A.

Historical and Personal.

OVER twenty years ago I commenced the study of the glacial deposits of the neighbourhood of Liverpool, and as the observations grew they came to embrace a considerable share of the drainage-basin of the Irish Sea.

I have personally inspected and kept full records of all of the important artificial excavations likely to throw light upon the subject, in addition to examining and making sections of the natural exposures of glacial drift which abound on the north-west coast of England, the coast of Wales, and in the river valleys draining into the Irish Sea, and to a lesser extent the drift on the east-coast of Ireland and the south of Scotland. These observations, together with others in districts outside the special area of my work, have been communicated to various Societies and Magazines.[1]

This being my record, and glacial geology having during my time gone through several phases, it may perhaps interest some of the readers of the Geological Magazine if I give a *resumé* of the views I have been gradually led to adopt.

[1] Proc. of Liverpool Geol. Soc. 1872–73, pp. 31, 32, pp. 42–65; 1873–74, pp. 50–72. Q.J.G.S. vol. xxx. p. 27–41, 1874. Proc. of Liverpool Geol. Soc. vol. iii. pt. 1, pp. 19–27, 1875. Geol. Mag. Dec. II. Vol. III. p. 480, 1876. Proc. of Liverpool Geol. Soc. vol. iii. pt. 3, pp. 241–43, 1877. Geol. Mag. Dec. II. Vol. IV. pp. 38, 39, 1877. Q J.G.S. vol. xxxiv. pp. 808–810, 1878; vol. xxv. pp. 679–681, 1879. Proc. of Liverpool Geol. Soc. vol. iv. pt. 1, pp. 64–89, 1879; vol. iv. pt. 2, pp. 139–153, 1880; vol. iv. pt. 3, pp. 216–233, 1881. Q J.G.S. vol. xxxviii. pp. 222–238, 1882. Trans. Geol. Soc. Glasgow, vol. vi. pt. 2, pp. 264–276, 1882. Proc. of Liverpool Geol. Soc. vol. iv. pt. 5, pp. 364–380, 1883. Q.J.G.S. vol. xxxix. pp. 83–132, 1883. Trans. of Liverpool Geol. Asso. vol. iii. pp. 93–101, 1883. Q.J.G.S. vol. xl. pp. 267–269; vol. xl. pp. 270–272, 1884. Proc. of Liverpool Geol. Soc. vol. v. pt. 1, pp. 74–83, 1885. Q.J.G.S. vol. xli. pp. 102–107; vol. xli. pp. 454–456, 1885. Proc. of Liverpool Geol. Soc. vol. v. pt. 4, pp. 377–379, 1888. Nature, vol. xl. p. 246, 1889. Proc. Liverpool Geol. Soc. vol. vi. pt. 2, p. 188, 1890; vol. vi. pt. 3, pp. 316–321, and pp. 333–4, 1891. Geol. Mag. Vol. VIII. Dec. III. p. 291, 1891. Proc. Geol. Soc. 1892.

On commencing the inquiry I had no idea whither my observations would lead me, and had no prepossessions. I quickly found, however, that I came into collision with one of the prevailing views —I had almost said dogmas—of the day.

The drift of the north-west of England had been arranged in a tripartite series consisting of, in ascending order, Lower Boulder Clay, Middle or Interglacial Sands and Gravels, and Upper Boulder-clay. My observations led me to believe that there were no sufficient grounds for such a geological division, and that the whole of the drift deposits were one glacial series from top to bottom. This view is now the one generally adopted, and also as I understand by the new school of glacialists who ascribe these deposits to the ploughing up of the Irish-Sea bottom by land ice. And this brings to my mind the fact that there has been a concurrent change of position on the part of the land-ice glacialists, for the old school believed in the occurrence of several interglacial periods and thought they could read the record of them in the deposits.

Until lately the preponderance of opinion among geologists who have studied the subject in the field was that the glacial deposits of the north-west of England were sea-bottom *in situ* and that the high-level sands and gravels indicated a submergence of the land in glacial times to at least 1400 feet.

The late Mr. Belt struck a new note when he boldly declared his belief that the high-level drift of Moel Tryfaen, in Carnarvonshire, had been pushed up into its present position by a sheet of land-ice traversing the Irish Sea.[1] Mr. Belt was listened to by few at the time, but in the whirligig of time he is now having ample revenge.

The late Professor Carvill Lewis, fresh from his experiences in America, revived this idea of Belt's in a modified form, and he also met with much opposition. Since his death the idea has fructified, and we now have an energetic band of geologists who are bent on nothing less than raising what was considered at first to be a "wild idea" into a geological dogma.

Whether there has been justification for this veering round of geological opinion it will be our object presently to inquire. In the meantime it is sufficient to suggest that it is not the province of geological enquiry to search out facts with the object consciously or unconsciously of running a theory, yet this is one of the many pitfalls against which geological reasoners have to be constantly on their guard.

The Glacial Drift of the Irish-Sea Basin and its System of Distribution.

Low-Level Boulder-clay and Sands.

The whole of the plains of Lancashire and Cheshire are, with the exception of local patches, covered with a mantle of Boulder-clay and sands of varying thickness, from a few feet in the more exposed localities to 160 feet in the deeper river channels. Both the clay and sands generally contain remains of mollusca in a more or less fragmentary condition.

[1] Nature, vol. x. pp. 25, 26, 1874.

The Boulder-clay is usually of a brown colour, sometimes unctuous, and contains boulders and fragments of Northern rocks from the maximum of over 20 tons down to the finest gravel. The boulders are to the extent of fully 50 per cent. either planed or striated or both, and sometimes on several faces. They are often considerably rounded and the finer gravel to be obtained by washing the clay is extremely waterworn. The sand, if separated from its clayey matrix, is much rounded, some of the grains being extremely polished.

The proportion of sand in the clay is much greater than would at first be thought, varying from 20 to 60 per cent. There is no geological difference between the Sands and the Boulder-clays, gradations from the one to the other can be met with though often they come together in sharp juxtaposition. If we trace these drift deposits up the valleys from the sea to the source of the rivers flowing in them, we find that the nature of the matrix of the drift lying in them is largely dependent upon the nature of the rocks the rivers traverse,[1] that is to say, the sandy and gravelly drift increases when the rivers above them flow through sandstones and rocky ground as in the Dee above Chester, and the Mersey and Irwell in the neighbourhood of Manchester, while the rivers flowing over the New Red Marl have brought down an unctuous clay like that deposited by the Weaver in the Buried Valley of the Mersey at Widnes. Thus, while the contained stones have come from the northward, with the possible exception of some sandstones and gypsum of the New Red Marl, the matrix consisting of clay and sand has to the larger extent come down the river valleys.

If we extend our observations further south into Shropshire we find a still greater development of sands. The drift by the Severn at Shrewsbury is nearly all red sand, the waterwashed *débris* of the New Red rocks. The erratics are still plentifully strewed about the county, and the well-known Eskdale granite and the grey granite of the south of Scotland, together with the andesitic rocks and volcanic ashes from the English Lake district are distinguishable. It is also a fact worth noting that on the plain of New Red Marl in Cheshire the drift sands and gravels are more developed than the Boulder-clay; often they lie directly upon the marl without the intervention of any Boulder-clay. I have observed this also in Shropshire.

High-Level Sands and Gravels.

At Moel Tryfaen, Carnarvonshire; at several places in Flintshire (Moel-y-Crio among them); between Minera and Llangollen, Denbighshire; at Gloppa, near Oswestry; at the Setter's Dog, near Macclesfield, and the Three-Rock Mountain, near Dublin, sands and gravels are found varying from about 1000 to 1400 feet above the sea-level. These sands and gravels contain shells of mollusca, speaking generally, of a similar *facies* to those fragments found in the Low-level Boulder-clay and sands. More perfect specimens have been found at these high levels, especially at Gloppa, by Mr. Nicholson, F.G.S., than

[1] Quart. Journ. Geol. Soc. vol. xxxix. pp. 83–132.

are generally met with in the Drift of the plains. Drift with shells is met with at various places at levels intermediate between the Low-level Boulder-clay and sands and the High-level sands and gravels. This Drift is usually laminated and current-bedded. At Moel Tryfaen, where I devoted a good deal of study to the Drift, there is a much greater preponderance of erratic stones, such as Lake District rocks and Scotch granites, flints supposed to come from Antrim, with an admixture of Carboniferous limestone which may be from Anglesey, and true Anglesey crystalline schists, than is contained in the Drift of the coastal plain which reaches from sea level up to the 400 feet contour. It is rather a remarkable fact that in this high-level Drift I found a piece of Shap Fells rock in the form of a rounded pebble identified by Mr. Alfred Harker, F.G.S., as from one of the numerous offshoots of the granite mass of Shap Fells. I mention this specially because during the whole time I have been observing I have never met with any Shap Fells granite on this side of the Pennine Chain, either in Lancashire, Cheshire, or North Wales.

The High-level sands and gravels of Tryfaen are overlaid on the eastern side of the excavations with a stony Till, evidently of local derivation, and containing nearly all local rocks. In one place a laminated bed of the sands inosculated with the Till, which also contained pockets of the sand. This is a very striking feature which has only been developed of late by the progress of the excavations. The stones contained in the sands and gravels are much waterworn, and there is a very much smaller proportion of them striated than what we find in the Low-level Boulder-clay. The grains of sand are also much rounded, waterworn, and polished. The Till also contains a proportion of these highly polished grains, and I found on washing some of the locally formed Till lying at a level of 800 feet above the sea, between The Rivals and Mynydd Carnguwch, that this also contained highly worn and polished quartz grains.

Drift of the Coastal Plain adjoining Tryfaen.

The Drift of the coastal plain adjoining Tryfaen is well exposed in coast sections, and consists more largely of material from the Snowdonian Range, both as regards boulders and the matrix, which is largely made up of the debris of slate rock and small slate flakes. An arched stratification is seen in some of the sections. The Till can be examined in numerous sections cut by the streams through the coastal plain, and a strict search almost everywhere yielded pebbles of Eskdale and other erratic granites, but seldom any fragments that could be described as "boulders."

Mechanical analyses of twenty samples of the Till from various localities, with one exception, yielded the rounded and polished quartz grains. In some cases I found small shell fragments, and ample evidence was accumulated that the drift of the coastal plain was an intimate mixture of the debris of the Snowdonian rocks and erratic material from the North, and in some cases from Anglesey.

Tracing the coast sections south-westwardly, they gradually put

on a more distinctively marine character, until at Nevin they assume the ordinary form of stratified drift-sands with shell fragments.

Significance of the polished quartz grains.

It will be necessary to turn aside here to point out the value I attach to the discovery of the prevalence of rounded quartz sand in the Till. I have been making a very exhaustive examination of sands from various parts of the world, and I find that contrary to received ideas marine sands are, on the whole, much more rounded than river sands. Indeed, river sands are generally little rounded, and, excepting in a river like the Amazon, I can find no parallel to the rounding of the grains of sand now dredged from the Irish Sea. Blown-sand of sand-dunes is not distinguishably more worn than the sand of the shore from which it is derived. When we find these highly-polished grains mixed up with angular and little worn fragments of Snowdonian rocks forming the bulk of the Till of the coastal plain, it is evident that they are of sea origin, and serve as certainly as the fragmentary shells of Molluscs to declare this fact. I consider this an important discovery, which may, taking surrounding conditions into consideration, help to settle many disputes as to the marine or freshwater origin of certain deposits. In the present case, of course, the rounded grains can tell us no more of how they got to the high-levels than the shells of the Molluscs, but more of this presently.

Distinction between the Marine Drift of the Plains and Mountain Drift.

Where the Marine Drift of the plains approaches a mountainous district, and the base can be seen, it usually lies upon a Till made up almost wholly of local materials derived from the mountains. In some cases the Marine Drift is absent. At various points along the coast of Merionethshire, and in Mid-Wales, the local Till is prominently displayed in coast sections, but where a large estuary valley opens out on to the coast, and sections display the nature of the Drift, we find, as in the neighbourhood of Towyn, distinctly Marine Drift in some places, and a mixture of Mountain and Marine Drift in others. The stony Till clings to the mountain sides, the Marine Drift comes in more strongly as it is distant from the mountains. This is a rule to which, personally, I have seen no exceptions.

Theories of Origin.

Most of the details upon which the preceding sketch is founded, with the exception of those relating to the North and Mid-Wales coast, have been published. It is very necessary that details should be published, but the Drift is so complicated that it is very difficult, if not impossible, to describe it geologically in the sense that other formations, both older and younger, can be described. It is this that has beset the path of most glacialists, and the attempts to reduce the Drift deposits to geological order and system have been the fruitful source of many failures.

So far as the Drift in the districts described in these pages is concerned, I long ago arrived at the opinion that no good could be got out of it—that is, no intelligible story—until we recognized it as one series of glacial deposits. On this principle, with the facts already stated before us, I propose to discuss the two opposing theories of the Glacial Drift, namely, the land-ice and the glacio-marine or submergence theory.

The Land-Ice Theory.

The present form of this theory postulates that during the period when the Drift, as we see it now, was formed, the relative levels of the sea and land were the same as now, and that a great ice-sheet advancing from the North over the Irish-Sea bottom ploughed out the deposits, re-arranged them, and pushing them before it, or conveying the materials frozen in the bottom of the ice, spread them over the lowland plains, and even pushed the sands and gravels with shells up to the high levels in the instances already named. To the obvious objection that the Drift bears indisputable signs of aqueous deposition, it is answered that the current-bedded sands and gravels are due to the washing from the melting of the ice at the termination of and underneath the Mer-de-glace.

Physical conditions involved.

Before describing what the deposits themselves have to say to this theory, let us picture to ourselves what it as a matter of physics involves. The gathering ground of such a glacier could at first only be the land area which, by the terms of the postulate, was the same as now. A glance at the Map of the British Isles is sufficient to show that the snow-field could not have been above double the area of the sea it had to displace. The ice-front advancing over the Irish Sea would have had an average length of one hundred miles. The waste by melting would have been enormous, so that an intensity of glacial conditions far in excess of those of Greenland of the present day would have to be granted.

Probable Effect on the Sea Bottom.

If, however, for the sake of discussing the question, we grant both the necessary conditions and their result, the ice-sheet, what would be the probable effect on the sea bottom? Unfortunately we have very little to guide us, for it is one of the weakest places in this theory that there are no analogous modern examples that we can appeal to. Even with those glaciers that displace the sea from an inlet like the Malaspina Glacier of Mount St. Elias in Alaska,[1] it is impossible to say what effect the glacier has had on any deposits that may previously have occupied the bottom. We know, however, that glaciers frequently over-ride loose deposits on land without displacing them, while in others they erode their beds. By the terms of the postulate Pre-glacial deposits must have occupied the Irish Sea before the over-riding of the ice-sheet bringing its load of

[1] Israel Russell,—National Geographic Magazine, May, 1891.

northern rocks along with it. A short Iceberg period must have preceded the final development of the Ice-sheet, even on this theory, and the products of this interval might get ploughed up and mixed with the products of the Ice-sheet. But what has become of the Pre-glacial deposits? I have examined most artificial exposures in Lancashire and Cheshire calculated to throw light upon the subject, but nothing in the form of a Pre-glacial deposit have I ever seen. The Alexandra Docks at Liverpool and the Docks at Garston gave grand opportunities for inspecting the rocky floor below tide level, the Mersey Tunnel also, in a lesser degree, but everywhere the glacial deposits rest directly on the bed-rock or the debris of the bed-rock. Borings at Widnes and well and shaft sinkings at various places in the Mersey Valley, all below the level of the sea, tell the same story. So far as we know, and I have seen nothing recorded to the contrary, the glacial deposits, with local variations, are the same from the top to the bottom of the series. The "Gully Gravels" lying in depressions down to 160 feet below the sea level are glacial, as shown by the rocks from which they have been derived being the same as those in the Boulder-clay. If, as is contended, the Drift of Lancashire and Cheshire is ploughed-up Irish-Sea bottom, it is very remarkable that it possesses such homologous characteristics throughout, and that the Pre-glacial deposits have been so thoroughly mixed with the materials brought by the land-ice that there is no distinguishable difference from the top to the bottom of the series. Not only are Pre-glacial deposits not to be found *in situ*, but not a scrap or shred of anything of the sort have I ever found embedded in or associated with the Boulder-clay.

Areal Distribution of Erratics.

Not only are the rocky materials contained in the Drift not arranged in any discoverable vertical order, but well recognized types of rock have a wide areal distribution. All over Lancashire, Cheshire, and Shropshire, Scotch and Eskdale granites are to be found in large boulders now scattered over the surface, while smaller boulders and pebbles of the same rocks are to be seen in the Drift itself. East and west they extend also from the Pennine Chain across Lancashire and Cheshire and along the coast of Wales to beyond Moel Tryfaen, in Carnarvonshire.

If all these boulders have been conveyed to these points by an ice-sheet, some reasonable explanation of how it was done ought to be given.

This hypothetical ice-sheet is supposed to have originated in Scotland, and to have been reinforced by glaciers from the north-west of England and the north-east of Ireland. Its course was over the bed of the Irish Sea to Liverpool Bay, at which place it divided into two lobes, one of which flowed onwards over Cheshire and Shropshire, and the other flowing westward, skirted the coast of Wales. Taking Criffel as the origin of the Scotch granite, it would form the apex of a triangular area over which this granite has been distributed, having a base—measuring east and west along the coast of Wales—as long as its sides.

The divergence of flow from Criffel will thus have been as great as 40 degrees, a very improbable thing to have happened with an ice-sheet traversing a basin fed from three sides. But when we consider that Lake District granites are found all along the same base, the improbability becomes to my mind an impossibility; for it would involve, first, a concentration and mixing of the two differently-derived rocks on or in the ice-sheet, and their after distribution in a fan-like form. This is omitting from consideration the further difficulty of the Antrim flints and other erratic rocks found over the same area.

High-Level Sands and Gravels.

Not only does the land-ice theory fail to explain the areal distribution of erratics, but we have the further difficulty to contend with that they are found at all levels up to 1400 feet above the sea. They are, as already pointed out, proportionally greater in numbers at the top of Tryfaen than in the Drift of the coastal plain of Carnarvonshire. As a question in dynamics it has never been shown what gradient would be required for an ice-sheet from the North thus to overpower the native glaciers of Snowdonia. The Welsh mountains are higher than any in the South of Scotland, so that to overpower the thrust of the ice northwards from a Snowdonian centre, and to deflect it south-westwardly, an enormous pile of snow would have to be concentrated in Scotland. A surface slope of half a degree would mean a depth of snow of over a mile at Criffel, plus the depth of snow on Tryfaen. The gradient of the Welsh ice-sheet, even if the top of Snowdon were bare, would be about 4 degrees from Snowdon to the top of Tryfaen. Putting it in the most favourable light for the land-ice theory, it would appear that even had the snow been two miles thick at Criffel, and in Wales reached only to the top of Snowdon, the Snowdonian glacier would have overcome the northern ice-sheet and have effectually prevented the landing of sea-bottom on a flanking spur of Snowdon, were such an event otherwise possible.

But there yet remains another difficulty; the deposits in question are admittedly *aqueously* deposited, so that on the ice-sheet theory the ice containing these sea-bottom remains must have melted and in such a manner as to have deposited the sands and gravels and boulders in a stratified mass on the summit of a hill 1400 feet above the sea-level. No one has grappled with this hydraulic difficulty. Similar reasoning applies to the Gloppa drift, which is 1100 feet above the sea and over 60 feet thick, probably 100 feet, as it has never been bottomed, and bears in its current-bedded stratification indubitable marks of aqueous deposition. There still remains a most important fact which must not be lost sight of in this connexion, namely, the presence of perfect and delicate shells. Since Mr. Nicholson, F.G.S., made his splendid collection of glacial shells on Gloppa, this difficulty has been emphasized. The fragmentary condition of most of the Drift-shells had been triumphantly pointed to as a convincing proof of the passage of an ice-sheet over them.

Now the odd thing happens that the largest collection of the most perfect shells are found just in the place where they are least wanted on the ice-sheet hypothesis. Obviously a safe means of transport must be provided, and this is supposed to be ensured by the working up of the sea-bottom material into the ice-sheet, and these delicate shells, solidly encased in ice, are pushed safely an unknown distance over the sea-bottom and then uphill and down dale to be eventually melted out and deposited upon the summit of a hill 1100 feet above sea-level.

There is yet another if a minor question of a physical nature that obviously requires answering. If the ice-sheet possessed sufficient power to force sea-bottom 1400 feet up the slopes of Snowdonia and within five miles of its highest centre, why should the snows from the mountains of Denbighshire and Flintshire be sufficient to divert one lobe of it over the plains of Cheshire, and the other along the Welsh coast?[1] Rather one would expect that the whole of Flintshire and Denbighshire would have been overwhelmed with an ice-covering marching irresistibly towards the Midlands.

Nor must we lose sight of the fact that near Macclesfield and on the Three-Rock Mountain, near Dublin, shelly sands and gravels have been found at a level of about 1200 feet above the sea. These places are on the same parallel of latitude and about 190 miles apart. If the transport was by land ice, the ice-sheet filling the Irish Sea must have had here a front of not less extension than 200 miles, and a minimum depth of say 2000 feet; but on this head we have very little to go by. We may very well ask how a snow-field in Scotland of a less width than the distance between these two places, even assisted by the bordering fields of England and Scotland, could have generated a glacier which not only displaced the water of the Irish Sea, but after spreading out 190 miles still maintained such an enormous depth as to be capable of forcing up sea-bottom laterally on either side to the height of 1200 feet; and in the case of the Three-Rock Mountain against the contributory glaciers of Irish origin.

Distribution of the Sands and the Matrix of the Boulder-clay on the Ice-sheet Hypothesis.

If the Drift occupying Lancashire, Cheshire, and Shropshire has been carried there by an ice-sheet it would show in its constitution little or no relation to the water-sheds in which it occurs; whereas, as I have shown, the contrary is the case. The clays would not preponderate as they do in the lower part of the basin of the Mersey, nor would the sands be found lying upon the New Red Marl as they do in Cheshire and Shropshire.

If an ice-sheet had passed over the Red Marl of Cheshire we ought naturally to look for a great mass of Boulder-clays further south; instead of this sands and gravels preponderate. The waste of the New Red Marl of Cheshire has flowed in a northerly direction

[1] Mr. Strahan shows that the Drift of the upper part of the Vale of Clwyd has travelled North-East.—Memoir of the Geological Survey, Geology of the neighbourhood of Flint, Mold, and Ruthin.

along its present water-shed lines and this is further indicated by the common presence of gypsum in the Cheshire Boulder-clay.

Presence of Materials and Fossils derived from the South-East.

In the Drift gravels at Wollerton, Shropshire, I found numerous flints, as also to the north of the patch of Shropshire Lias, and at Market Drayton in Shropshire. That these flints have travelled from the south-east I have little doubt. Liassic, Gault and Chalk fossils have been recorded in the Gloppa Drift by Nicholson;[1] Liassic fossils by myself at Wollerton, Shropshire. *Waldheimia obovata* is recorded from the Drift-sand at Wellington, Shropshire, by Dr. Callaway. In the Old Gravels above Wigmore Lake, not far from Ludlow, the Rev. T. T. Lewis obtained some specimens of Lias gryphytes (Record of the Rocks, p. 177). Beete Jukes says (Memoir of South Staffordshire Coalfield, second edition, pp. 207, 208) that Chalk fossils often abound in sand and gravel at Wolverhampton, and between it and Shiffnal, and sometimes broken fossils of the Lias and Oolitic formations, and seem therefore rather derived from the east than the north. Mackintosh describes coal found in Drift-sand, near Corwen, which he, with a good show of reason, infers has come from near Ruabon or from east to west. "Mr. Molyneux has noticed what he describes as a remarkable tract of Chalk-flints stretching across the high ground of Hanbury Woodend (north-west of Burton-on-Trent) running east and west" (Deeley, Q.J.G.S., vol. xlii. p. 459). Charnwood Forest rocks are found in the Boulder-clay of Nottingham, and must therefore have travelled in a northerly direction (*ibid.* p. 480).

I have myself found pre-Cambrian rocks of the Wrekin in Boulder-clay immediately to the north of the Wrekin.

This drift of materials is either across or directly opposite to the direction of the supposed flow of the ice-sheet. I have no doubt the list could be much extended, and these facts cannot be reasonably accounted for on the land-ice hypothesis.

The Glacio-Marine or Submergence Theory.

While the land-ice hypothesis, supported by much ingenious reasoning, based upon imagination rather than facts, fails to explain numerous phenomena, the old idea of submergence offers a much simpler and more reasonable explanation of the drift phenomena I have described in these pages.

The dispersion of erratics, the presence of marine shells, the wear of boulders, and the rounding of sand grains, are all consistent with Glacio-Marine deposition. The preponderance of far-travelled erratics in the high-level Marine Drift of Tryfaen is suggestive of the same cause. I have found crystalline schist from Anglesey in this drift in a water-worn condition, but the Lake District and Scotch granites greatly preponderate. Yet Anglesey is directly in the path of the hypothetical ice-sheet, and should therefore have

[1] Mr. Nicholson tells me he found one of the Lias fossils himself in the Drift, and that although the others were found by the workmen, he has no reason to doubt their genuineness.

contributed the larger store of rocky materials. The granite centres on the submergence theory would, being at a greater elevation and casting off glacial ice, continue supplying granite, boulders, and pebbles for a much longer period than Anglesey. Thus, although more distant in their derivation, the preponderance of granites on Tryfaen is satisfactorily explained.

I have already in various papers expressed my opinion that the Low-level Boulder-clay and sands have been mainly formed in shallow waters when the subaerial denudation of the land contributed largely to their formation.

Their distribution is consistent with this view, but not with the view that the whole of their materials have been pushed along the land from the northwards, and are mainly composed of ploughed-up Irish-Sea bottom. It used to be a principle among glacialists that the proof of any deposit being "ground moraine" was the preponderance of local materials in it. Now the carrying powers of land-ice have been theoretically extended to include almost every glacial deposit in the category of ground-moraine, so that the boulders forming the great bulk of the rocks in Boulder-clay hundreds of miles away from their origin, and among which local rocks may be searched for in vain, form no difficulty to the modern glacialist.

But, say the supporters of the land-ice theory, the high-level drifts are uncommon and sporadic, whereas on the submergence theory they ought to be common and general. I confess myself unable to see, if there be any truth in this argument, why it should not equally apply to the deposits of an ice-sheet. The Drift has been spread pretty evenly and generally over the low-lands, which is consistent with glacio-marine deposition. It has been spread less generally and more irregularly over the hilly districts, but yet much more largely than the land-ice theorists would have us believe. It must be an exceptional thing for shells to be present in these loose deposits, yet nothing that has them not is admitted as marine-drift. Again, on the low-lands numerous artificial excavations are constantly showing us the nature of the subsoil, while on the hills we have to wait for the occasional opening of a slate quarry or a gravel-pit before we can ascertain what the superficial covering is composed of or what its depth is. It was the accident that between 30,000 and 40,000 tons of sand and gravel were required for the Vyrnwy filter-beds that discovered the existence of these extensive and thick beds of sands and gravels with their remarkable molluscous remains, previously unsuspected. I have little doubt that these examples will multiply as further engineering works are carried out. Quarries are not so likely to discover these beds, for quarrymen avoid such places, as the expense involved in removing the "fay," as the overlying loose deposits are called, lead their operations to where there is less cover on the rocks.

Another objection urged against the submergence theory is the supposed impossibility of shells representing such diverse conditions being congregated in one place. Strange to say, Forbes was the first to raise this difficulty with respect to the shells found by Trimmer on Tryfaen. As a matter of fact, I have in my collection the whole of

the list (except one species) picked off the Crosby shore either by myself or my sons. Species and genera the most diverse in habitat are thrown up on the shore, and the same may just as readily have happened in the glacial sea in glacial times. Again, it is urged that there are shells whose habitats now are southern, mixed up with boreal types, and that they could not have lived together in the same sea. The only shell of southern type that has anything approaching a wide distribution in the Drift is *Venus chione*, and that is now found living in Carnarvon Bay. The southern types of shell appear to be more frequent (though only two species are recorded from Gloppa) in the High-level sands and gravels than in the Low-level Boulder-clay and sands; and this is consistent with the amelioration of the climate which set in with the submergence. Some Tertiary shells are now only boreal in their habitats, notably *Tellina calcarea*,[1] a common fossil of the Scotch drift; yet it hardly would be contended that any part of the Tertiary period was colder than the present. The fact is we are ignorant of the causes that govern the distribution of molluscs, and are not in a position to say that it is solely temperature. At Cape Cod, in latitude 42°, arctic and southern forms are now dredged up alive from the same bottom. Finally, the great majority of the mollusca of the Drift are now found living in the Irish Sea. Not only so, but the most common species of the Drift of the north-west of England are the most frequently found now on the sandy shores of Lancashire. If a few boreal forms were introduced among the living molluscous fauna of the Irish Sea, and two or three southern types less freely, we should have a pretty close reproduction of the fauna of the Drift. The shells of the Drift are little distinguishable in condition from those of recent molluscs, which I believe are lineal decendants of those that occupied the Irish Sea in Glacial times.

The remarkable equality of maximum level of the high-level sands and gravels with shells scattered over an area 190 miles wide from east to west points towards deposition by the sea rather than by land-ice.

With this I must for the present conclude, for though I have far from exhausted the illustrations and arguments gained in the field during the last twenty years, the reasonable limits of space prevent me from expanding them. Enough, I trust, has been said to show that the Glacial deposits, being fairly interrogated, speak strongly in favour of a glacio-marine origin.

VI.—A Sand-Pit at Hill Morton, near Rugby.

By the Rev. P. B. Brodie, M.A., F.G.S.

During a recent visit of the Warwickshire Naturalists' and Archæologists' Field Club to Rugby, a very interesting section was examined at Hill Morton. It consists, for the most part, of brown and light-coloured sands, exposed near the London and

[1] "Occurs in every Tertiary bed up to the Red Crag" (Jeffreys, British Conchology, vol. ii. p. 390).

North-Western Railway, at least 50 feet thick, often false-bedded, the laminæ of deposition being very irregular, interspersed here and there with numerous small pebbles, many of which are sandstone, none very large, though all are more or less rounded, and including much flint. Amongst these were many Lias Gryphæas (chiefly *G. incurva*), and some Cretaceous, Oolitic and Liassic pebbles with occasional fossils; but apparently not many ancient rocks, some of which may possibly be Carboniferous. There are also here and there traces of Carbonaceous matter. The interest in the section consists in the accumulation of such a mass of soft sand, which occupies a ridge of considerable extent both in length and breadth, on high ground, and is seen at other places in and near the village, and can hardly be considered to belong to the *Drift* properly so-called, and might almost be supposed to be referable to some later Tertiary deposit just anterior to the earliest Drift epoch. No recent shells of any kind were noticed, so that it is impossible to say whether the sand was fluviatile or marine.

It is not easy to determine whence such a mass of fine sand was derived, though it may perhaps have originated from the denudation of some of the sandy beds of the Middle Lias not far off, or from some of the more arenaceous Oolitic rocks. If, which is not improbable, the Chalk and Greensand formerly extended over this area, and have since been entirely removed, the Cretaceous sands would have supplied ample material. Coarse Drift also seen elsewhere, in the same position, but quite distinct from the sand below, occurs at the top overlying the sand.

As this section has not been recorded in the Geological Survey Memoir of the District, it seems to be worthy of notice.

REVIEWS.

I.—The Great Earthquake of Japan, 1891. By Professor John Milne, F.R.S., F G.S., and Professor W. K. Burton, C.E. Illustrated by 29 Plates and a Map of Japan prepared by K. Ogawa. Long folio, pp. 71. (Lane, Crawford, & Co., Yokahama; and E. Stanford, Charing Cross, London. Price £2 2*s*.)

TO Professor John Milne belongs the credit of placing the study of seismology in Japan upon a sound scientific basis. By his earnestness and enthusiasm he created the Seismological Society of Japan, which has already published sixteen volumes of Transactions. He has induced the Japanese Government to establish stations of observation in various parts of the Empire and to have notice of all shocks telegraphed to Prof. Milne at Tokio to be duly recorded and studied. Largely at his own expense, and aided by grants from the British Association, he has set up seismographs invented by himself and Mr. Gray, in Tokio and other places. He has spared neither labour nor expense to carry on his researches, and the record of the great Earthquake of last year is now presented to

us as a handsome album, measuring 16 inches by 11 inches, with text by himself and Prof. W. K. Burton, illustrated by thirty plates reproduced from actual photographs, vividly portraying the scenes of this terrible calamity, each plate accompanied by descriptive letterpress. The following extracts are taken from the authors' introduction :—

"Japan is a land of Earthquakes and Volcanoes. Every year its inhabitants are shaken by at least five hundred shocks, and at intervals—several of which fall within the memory of the living—some part or other of the Empire is visited by a terrible catastrophe.

"When nature thus exerts itself cities are rocked like ships upon the ocean, and it is some time before equilibrium is restored. There is a mighty effort, as if a mountain range had escaped the pressure that held it in its crumpled form, and the country is suddenly thrown into the most violent oscillation. Complete relief, however, is not obtained at once; and, for some months, minor yieldings announce themselves with subterranean thunderings and smaller shakings on the surface. In these years one or two thousand shakings are added to the average five hundred."

"When we are not shaken by Earthquakes, certain sections of the country are threatened by volcanoes. Only a few years ago a terrible explosion took place on the side of the grass-covered Baudaison, and in less than ten minutes a tract of country measuring thirteen miles by ten was submerged beneath a sea of earth and boulders at least one hundred feet in depth. Hamlets and farms were buried, and nearly 600 people lost their lives."

"In Japan there are at least three lines of weakness through which volcanic forces have forced openings, and around these the ejected material has built up cones. The first of these lines—which is at least 1000 miles in length—comes from Kamchatka through the Kuril Islands and Yesso down Nippon. Here it is met by a second line about 1500 miles in length, almost at right angles, which runs through the Bonin Islands to the Ladrones in the Pacific. The third comes up from the Philippines through Formosa, to the centre of Kinslim, where it terminates in Asosan, a volcano with a ring-formed crater ten miles in diameter. In the middle of Japan there are no volcanoes, but severe Earthquakes have been as frequent there as they have been in other portions of the country. The greatest frequency is along the east coast; and these disturbances, which are of daily occurrence, do not come from the volcanoes, nor does their frequency show any relationship to volcanic action as exhibited at craters." . . . . . .

"The vapour to which we now look as being a possible cause of Earthquakes is that of water. By capillary action, water soaks downwards to heated regions, and the resulting steam we know to be the motive power at our volcanoes. The earth's crust not being sufficiently strong to withstand the increasing subterranean pressure, whole mountains have been dissipated as dust and boulders, and although a great portion of the force of the explosion has been spent in the creation of air-waves, earthquakes of considerable magnitude

have been produced. Similarly we can conceive of subterranean explosions as the producers of earthquakes. Certainly the fact that many earthquakes occur in volcanic countries, and near the ocean where we therefore have both heat and moisture, support such a view."

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"The great disturbance which we illustrate, occurred about the centre of Japan, in the Prefectures of Aichi and Gifu. The severely shaken district, in many portions of which the destruction of buildings and engineering works was complete, extends over 4200 square miles. The area in which brick buildings were affected reached as far as Tokyo to the east, and Kobe to the west, or over an extent of country of 4400 square miles. The disturbance was, however, felt from Sendai in the north to Nagasaki in the South, or over an area of 92,000 square miles, and had Japan been surrounded by land instead of water, the land area shaken would have been about 400,000 square miles, Delicate instruments may possibly have been affected at the Antipodes. Effects were noticed in Shanghi."

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"If we were asked whether the Gifu-Nagoya plain was a place where earthquakes were frequent, we must reply in the affirmative. In Japan there are some seven hundred stations where earthquakes are observed, and from several of them situated on the Gifu plain we find that, in the six years from 1885 to 1890 the number of shocks recorded in that district were respectively, 9, 4, 10, 12, 15, and 36; whilst in the corresponding years in Tokyo, where accurate records are taken with seismographs, the numbers were 51, 55, 80, 101, 115, and 93."

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"Standing on one of the hills, which form the margin of the devastated area, a vast plain, covered with rice-fields dotted with clumps of trees and hamlets, and streaked with the silvery bands of the four great rivers which cross it, stretches as far as eye can reach. From the western side of this plain—which supports a population of perhaps 800 to the square mile—one sees towards the south the islets and promontories of Owari Bay, before one the turrets of the castles rising through the blue smoke of Nagoya and Ogaki, beyond which comes the gently sloping uplands forming foothills to dark green mountains."

"The Nagoya-Gifu plain is one of Japan's great gardens, but it has been devastated. A disturbance occurred in the Mino mountains, and at once an area, greater than that of the Empire of Japan, became a sea of waves, the movements being magnified on the surface of the soft alluvial plains. In Tokyo, more than two hundred miles from the centre of the disaster, the ground moved in long easy undulations, producing in some persons dizziness and nausea, the movement being not unlike what we might expect upon a raft rising and falling on an ocean swell. Near to its origin the waves were short and rapid, cities were overturned, the ground was fissured, small mud volcanoes were created, and the strongest engineering structures were ruined. About ten thousand people

lost their lives, fifteen thousand were wounded, and one hundred thousand houses were levelled with the plain, whilst almost every building in the Meozoseismal area was shattered. From the effects that have been produced upon huge engineering structures we must conclude that the earth-movements in Mino at the time of the great earthquake were at least equal to any movements recorded in the annals of seismology."

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A glance at the fine series of plates, reproduced from actual photographs (all executed in Japan by native artists), conveys a wonderful and realistic idea of the destruction wrought by this earth-movement on the works of man, and one can more easily realise what would be the effect of even a slight earth-movement to such a city as London with its thousands of houses, crowded upon one comparatively small area, and its vast and complex system of drainage, and gas and water-mains, to say nothing of its underground railways burrowing even beneath the Thames! How thankful ought we to be to the Japanese Government who have, by their earnest efforts to do honour to Prof. Milne, succeeded in detaining him upon their hospitable, though tremulous, shores; for had he decided to pay the same careful attention to the seismology of this country, our lives would have been passed in a continual state of anxious unrest, hoping for the best, yet fearing the worst continually.

H. W.

II.—Das Norddeutsche Unter-Oligocän und seine Molluskenfauna. Von A. von Koenen. Lieferung III. (Abh. Geol. Specialkarte v. Preuss. u. d. Thüringischen Staaten, Band X. Heft. 3, Berlin, 1891.)

THE third part of this valuable work on the Mollusca of the Lower Oligocene beds of Northern Germany is devoted exclusively to the consideration of the Naticidæ, Pyramidellidæ, Eulimidæ, Cerithidæ, and Turritellidæ.

Of the Naticidæ, Professor von Koenen recognizes four genera—*Natica*, *Naticina*, *Ampullina*, and *Sigaretus*.

The Pyramidellidæ mentioned include the genera *Syrnola*, *Eulimella*, *Odontostoma*, and *Turbonilla*. It has recently been shown that *Syrnola*, A. Adams, 1862, is synonymous with *Obeliscus*, Humphrey, 1797, and the latter name, therefore, is to be preferred.

The Eulimidæ described comprise the genera *Eulima* and *Niso*.

The Cerithidæ occupy a large section of the work, and the peculiar manner in which the author has dealt with this portion of his subject is worthy of note. He recognizes the fact that the genus *Cerithium*, as usually understood by the older malacologists, includes a variety of diverse forms, many of which have but little in common with the type of the genus, and, years since, were separated from it as being distinct genera; yet he calls them all *Cerithium*. Of these, *Bittium*, *Cerithiopsis*, and *Lovenella* occur in the beds in question. The author seems to be quite aware of the existence of these newer generic names, and groups the species together accordingly. Thus,

the genus *Bittium*, Gray, is said to be represented by *Cerithium granuliferum*, v. Koenen. Why not call the mollusc *Bittium granuliferum*? The name *Lovenella*, Sars, 1878, should be replaced by *Cerithiella*, Verrill, 1882, the first-mentioned being pre-occupied by *Lovenella*, Hincks, 1868. The other genera of the Cerithidæ mentioned by the author are *Triforis*, *Aporrhais*, and *Mesostoma*, though why the two last mentioned are so grouped we are at a loss to understand. *Aporrhais* does not in any way resemble any member of the Cerithidæ with which we are acquainted, and it is usually placed in the vicinity of the Strombidæ, near *Rimella* and *Rostellaria*—by some authors it is included in the Alata. The name *Mesostoma*, Deshayes, 1861, is occupied by Ant. Dujès, 1830, and even if it were not, its true name by priority should be *Cerithioderma*, Conrad, 1860. Moreover, it is now usually classed with the Trichotropidæ.

Of the Turritellidæ, Prof. von Koenen recognizes the genera *Turritella*, *Mathilda*, *Scaliola*, *Vermetus*, and *Siliquaria*. He includes the Scalaridæ as a sub-family of the Turritellidæ! *Cirsotrema* and *Acrilla* are mentioned as sub-genera, though of what genus, or genera, the author does not very clearly show. The name *Scalaria*, Lamarck, 1801, is used instead of *Scala*, Humphrey, 1797; the latter has been preferred for some time in America, and adopted in recent works in this country. *Crassiscala*, *Clathroscala*, and *Acirsa* occur, and together with the sub-genus *Acirsella* complete the list. The inclusion of the Scalaridæ (or rather Scalidæ, as they should be termed) amongst the Turritellidæ is much to be regretted in face of the writings of M. de Boury and others, on the systematic position of the group.

In spite of these shortcomings, however, the work will be welcomed by all malacologists and geologists who desire to become more perfectly acquainted with the fauna with which it deals. The large number of new specific forms described, and the careful manner in which they have been diagnosed and illustrated, are features the value of which must not be overlooked. G. F. H.

III.—Beiträge zur Kenntniss der fossilen Radiolarien aus Gesteinen der Trias und Palæozoischen Schichten. Von Dr. Rüst in Hanover. Palæontographica, Bd. XXXVIII., 3–6 Lieferung, März, 1892; pp. 107–192. Mit. Taf. VI.–XXX.

Contributions to the Knowledge of Fossil Radiolaria from the Triassic Rocks and from Palæozoic Strata. By Dr. Rüst.

DR. RÜST'S researches on the Radiolaria in the Lower Mesozoic and the Palæozoic strata have proved as fruitful as those which he has previously made on this group in the Cretaceous and Jurassic rocks. In this memoir no fewer than 247 species of Radiolaria from the Palæozoic, and 20 species from the Triassic rocks, are described; a remarkable result when it is considered that up to two years since these organisms were scarcely at all known in Palæozoic

strata. The Radiolaria mainly occur in jaspers, chert or hornstone, siliceous shales and slates, and siliceo-calcareous beds; but in the numerous Palæozoic limestones examined by the author, they are extremely rare and in bad preservation. In some of the siliceous rocks they are so abundant as evidently to form the main portions of the substance of the rock; as a rule they are, however, too poorly preserved to permit of specific determination; but here and there specimens are met with, usually in the form of hard nodular concretions containing phosphatic, carbonaceous and manganese ingredients, in which the structural details of the Radiolaria are wonderfully shown, not infrequently owing to their being stained by these accessory materials. Strange to say, these Palæozoic Radiolaria are in better condition than any yet known from Mesozoic rocks; and they are generally larger and possess stronger and more complicated tests than the Mesozoic species, thus more nearly resembling Tertiary and living forms. In the radiolarian-bearing rocks there are, occasionally, sponge-spicules, and in some of Silurian age, fragments of graptolites, but calcareous fossils are absent.

The Triassic Radiolaria were principally obtained in beds of chert of Middle and Lower Muschelkalk age, in the Tyrol, and at Felsö Eörs in Hungary. In the Palæozoic rocks the greatest development of Radiolaria is met with on the lowest horizon of the Carboniferous limestone, beginning at the Culm and reaching downwards to the Upper Devonian. In the Harz, Hessen and Waldeck, they are present in dark carbonaceous siliceous shales, jaspers and chert, they also have been met with in the Ural and possibly also in Sicily, in rocks of the same horizon. From these various localities 155 species have been described. In the Upper Devonian rocks of the Hartz the Radiolaria occur in siliceous shales containing manganese, but without carbonaceous material; whilst in the Lower Devonian of the Ural they are present in red jasper, and on the same horizon in Hessen in siliceous shale, which the author considers a true radiolarian mud. Altogether from Devonian strata 64 species have been recognized.

In rocks of Ordovician or Lower Silurian age Radiolaria occur in siliceous shales at Langenstriegis, in Saxony; Bohemia; Scotland; and more especially at Cabrières in Languedoc, where they are excellently preserved in phosphatic concretions loosely imbedded in the shales. No comparison is made of the forms in the Scotch chert with those from other localities, and no mention whatever of those described in a paper published nearly two years since. In all 26 species are described by the author from Lower Silurian beds; with the exception of three forms, they have been derived from the Cambrières concretions. At a still lower horizon, in the Cambrian rocks of Sonneberg, in Thuringia, Radiolaria are present, but their specific characters cannot be distinguished.

Out of 109 genera in which these Palæozoic and Triassic forms have been ranged, but two only are new. The 247 Palæozoic species fall under the following orders in Haeckel's classification of the group: Spheroidea, 81 sp.; Prunoidea, 48 sp.; Discoidea, 52 sp.;

Cyrtoidea, 55 sp.; Stephanoidea, 8 sp., and Larcoidea 3 species. Only 13 species are common to Jurassic and Cretaceous rocks.

The author seems justified in considering these siliceous radiolarian rocks as deposits of deep-sea character, comparable with the radiolarian oozes of the present oceans, and it remains for those who oppose this view to show wherein the difference consists.

In the accompanying 25 plates excellent illustrations of the new forms are given, enlarged to the scale of 300–450 diameters. It would have been better if the scale of enlargement had been given with each figure, for comparisons made with the measurements given in the text show that in some cases they are drawn on a much smaller scale than that stated.

Dr. Rüst deserves the thanks of all palæontologists for his arduous researches on the Mesozoic and Palæozoic Radiolaria, which are completed with this memoir. Not the least difficulty in this pursuit is to obtain material, for siliceous rocks, like those containing Radiolaria, have until lately been considered as barren of fossils, and consequently have been unnoticed by collectors. The preparation of microscopic sections likewise involves considerable labour, and the author states that 5000 were made for the purpose of this Memoir. The study of these small organisms is only now beginning, and judging from the fact that in a single hand-specimen of rock in which the forms are well preserved, fifty new species may be found, there can be little doubt that in number and variety fossil Radiolaria will prove in nowise behind those living at the present day.

G. J. H.

IV.—Anzeichen einer interglaziären Epoche in Central-Russland. (Umgebungen des Dorfes Troïzkoje, Gouv. Moskau.) Von N. Krischtafowitsch. Bulletin de la Soc. impériale des Naturalistes de Moscow, N.S. Tome IV. 1890, No. 4, pp. 527–547.

Indications of an Interglacial Period in Central Russia. (Village of Troizkoje, near Moscow.) By N. Krischtafowitsch.

IN the high banks on the right side of the Moskwa river, at the village of Troïzkoje, about 10 kilomètres from Moscow, there are exposed some beds of lacustrine derivation containing numerous plant and animal remains, including bones of the Mammoth, which are covered by typical Glacial deposits, and on this account have been supposed to be of pre-Glacial age, and compared with the Forest Bed of Norfolk. This lacustrine formation has been studied by several well-known Russian geologists during the last fifty years, and lately the author of this paper, accompanied by Professor and Madame Pavlow, made a more thorough examination of the locality, and by removing the débris at the base of the river bank ascertained that the lacustrine beds rested on glacial materials of the same character as those above them, thus showing that they were inter-Glacial instead of pre-Glacial in age. It had been assumed by previous writers that the fresh-water beds rested directly on the Upper Jurassic sandstones which form the bed-rock in this region.

The following gives the succession of the beds as shown in the river bank and on the sides of a lateral ravine:

(*a*) Surface layer of humus.
(*b*) Yellow sand with thin partings of clay, 1½ m.
(*c*) Yellowish-grey sands with erratic blocks of Northern origin, from Finland, Olonetz, &c., 3½ m.
Lacustrine deposit. (*d*)
I. A reddish-brown clay bed, becoming yellowish-grey below.
II. Greenish-brown and grey beds.
III. Very sandy, dark-green and grey beds with layers of turf.
(*e*) Thin layer of reddish-brown loam.
(*f*) Reddish-brown, yellow, and clear sands with erratics of Northern rocks; in the upper portion sandy concretions with crystalline erratics.
(*g*) Coarse glacial sand.
(*i*) Red ferruginous sandstone with erratics of crystalline rocks.
(*k*) Jurassic beds (?) upper Volga horizon.

The lacustrine beds are shown for a distance of about 100 mètres, and they have a maximum thickness of about 12 mètres. In some places organic materials are so abundant in the beds that they resemble lignite, and are readily combustible. The plant-remains in them include leaves of *Quercus, Salix, Alnus, Betula, Corylus, Acer, Pinus sylvestris, Nuphar luteum,* and mosses. Fresh-water species of diatoms are also extremely abundant The plants are similar to those now living in the same area, but some forms are more abundant than at present, which leads the author to conclude that the climate of that time was somewhat milder and moister than that of to-day. The animal-remains consist of wing-cases of beetles, shells of *Anodon,* and teeth, bones and scales of fishes, principally pike and perch. The Mammoth skeleton was found in an upright position; partly in the middle (II.) and partly in the lower bed (III.) of the deposit. From the remains it would appear that the flora and fauna of these inter-Glacial beds were substantially the same as at present, and that only the large Mammoth has disappeared.

The author has found traces of similar lacustrine materials at a locality 3 kil. distant from Troïskoje, and in the river banks of other places in Central Russia deposits are known which will probably prove to be of the same age as those described.

The evidence of the deposition of these beds and of a temperate climate, in a period intermediate between the formation of beds of glacial character, seems clear and indisputable, and there can be no doubt of their similarity to the inter-Glacial beds of Germany and Western Europe. G. J. H.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—May 25th, 1892.— W. H. Hudleston, Esq., M.A., F.R.S., President, in the Chair.—The following communications were read:

1. "On *Delphinognathus conocephalus* (Seeley) from the Middle Karoo Beds, Cape Colony, preserved in the South-African Museum, Capetown." By Prof. H. G. Seeley, F.R.S., F.G.S.

The skull described in this paper is believed by Mr. T. Bain to

have been collected by himself near Beaufort West. The preservation of the specimen leaves something to be desired, but notwithstanding defects the skull belongs to a most interesting Anomodont, indicating a new family of fossil Reptilia.

The skull is fully described in the paper, and its relationships are discussed. The author has already given reasons for regarding *Ælurosaurus felinus*, *Lycosaurus curvimola*, and their allies as referable to a suborder *Gennetotheria*, which is nearly related apparently to the *Pelycosauria*, and lies midway between the typical *Theriodontia* and the *Dicynodontia*. It is to this suborder that *Delphinognathus* may be referred, though it forms a family-type distinct from the *Ælurosauridæ*, distinguished by the conical parietal with a large foramen, the anterior supra-condylar notch in the squamosal bone, and other modifications of the skull and teeth.

2. "On Further Evidence of *Endothiodon bathystoma* (Owen) from Oude Kloof, in the Nieuwveldt Mountains, Cape Colony." By Prof. H. G. Seeley, F.R.S., F.G.S.

Two bones found by Mr. T. Bain at Oude Kloof, consist of the left ramus of the mandible and what the author regards as the left squamosal bone of *E. bathystoma*. The small cranial fragment preserved shows that the cerebral region probably conformed to the type of skull seen in some of the Dicynodonts.

A description of the remains is given, and the author notices that the form of the articular condyle indicates a difference from *Dicynodontia* and all other *Anomodontia* hitherto described; it implies an oblique forward inclination of the quadrate bone—a character important in defining the suborder *Endothiodontia*. All the characters of the dentition of the animal suggest near affinity with the *Theriodontia*, especially the long lanceolate teeth strongly serrated.

3. "On the Discovery of Mammoth and other Remains in Endsleigh Street, and on Sections exposed in Endsleigh Gardens, Gordon Street, Gordon Square, and Tavistock Square, N.W." By Henry Hicks, M.D., F.R.S., Secretary of the Geological Society.

In this paper the author gives a description of the deposits overlying the loam in which the remains of the Mammoth and other animals were found in Endsleigh Street, N.W. Under about 6 feet of made ground there was about 10 feet of a yellowish-brown clay containing flints and much 'race.' Below the clay there was about 5 feet of sand and gravel, and under this about 1 foot of clayey loam, in which most of the bones were embedded. This loam contained many seeds, recognized by Mr. Clement Reid, F.G.S., as being those of plants usually found in marshy places or ponds and having a range at present from the Arctic Circle to the South of Europe. A list of the bones found is given by Mr. E. T. Newton, F.G.S., of the Museum of Practical Geology, Jermyn Street, who describes them as being those of one full-grown Mammoth, of another about half-grown, of the Red Deer, the fossil Horse, and of a small rodent.

The author gives sections through Endsleigh Street and along the southern side of Endsleigh Gardens, and shows that where the

bones were found there was a distinct valley in the London Clay, running in a direction nearly due north and south, the inclination of the valley being towards the north. The London Clay reached nearest to the surface towards St. Pancras Church and in Upper Woburn Place, the total thickness of the overlying deposits and the made ground there being only about 12 feet.

Other sections, given along the southern side of Tavistock and Gordon Squares and through Gordon Street and the western side of Gordon Square, show varying thicknesses of the deposits, overlying the uneven floor of London Clay, of from 16 to 21 feet; the greatest thichness here is found at the north-western corner of Gordon Square.

Seeds were also discovered in a loam near the bottom of Gordon Street, at the same horizon as that containing the mammalian remains, and some shells were found in a band of sandy clay under a calcareous deposit, about half-way down the western side of Gordon Square.

The Author says that the deposits above the mammaliferous loam overlying the London Clay in this area cannot be classed as post-Glacial river-deposits, but must be considered as of Glacial origin. The animals, therefore, which evidently died on the old land-surface where their remains were found, lived there early in the Glacial period.

4. "The Morphology of *Stephanoceras zigzag*." By S. S. Buckman, Esq., F.G.S.

Material which has come into the author's possession throws light on the developments of *Stephanoceras zigzag*, and such developments seem to supply missing links in the connexion of Bathonian and Bajocian species.

The author separates the developments of *S. zigzag* into three series, and discusses the allied forms of each.

II.—June 8th, 1892.—W. H. Hudleston, Esq., M.A., F.R.S., President, in the Chair. The following communications were read:

1. "The Tertiary Microzoic Formations of Trinidad, West Indies." By R. J. Lechmere Guppy, Esq. (Communicated by Dr. H. Woodward, F.R.S.)

After giving an account of the general geology of the island, and noticing previous memoirs devoted to that geology, the author describes in detail the characters of the Naparima Beds, to which he assigns an Eocene and Miocene age. He considers that the Nariva Marls are not inferior to but above the Naparima Eocene Marls, and are actually of Miocene date.

Details are given of the composition and characters of the 'argiline,' the foraminiferal marls occasionally containing gypsum, and the diatomaceous and radiolarian deposits of Naparima.

The Pointapier section is then described, and its Cretaceous Beds considered, reasons being given for inferring that there was no break between the Cretaceous and Eocene rocks of the Parian area.

Detailed lists of the foraminiferal faunas of the marls are given, with notes.

The author observes that the Eocene molluscan fauna of Trinidad shows no near alliances with other known faunas, thus differing from the well-known Miocene fauna of Haiti, Jamaica, Cuba, Trinidad, and other localities. Only one mollusc is common to the Eocene and Miocene of the West Indies. The shallow-water foraminifera, are found in both Eocene and Miocene, whilst the deep-water foraminifera are nearly all of existing species.

It would appear that during the Cretaceous and Eocene periods a sea of variable depths (up to 1000 fathoms) occupied the region now containing the microzoic rocks of Trinidad, whilst a mountain-range (which may be termed the Parian range) extended continuously from the north of Trinidad to the littoral Cordillera of Venezuela, forming the southern boundary of the Caribbean continent, and possessing no large streams to transport mechanical sediment into the Cretaceo-Eocene sea which opened eastward into the Atlantic.

An Appendix by Mr. J. W. Gregory deals with the microscopic structure of the rocks.

2. "The Bagshot Beds of Bagshot Heath (a Rejoinder)." By the Rev. A. Irving, D.Sc., F.G.S.

The author maintains that the northerly attenuation of the Lower Sands and of the 'green-earth series' between the two principal brick-clays of the district is an established fact. He insists on the value of the Wellington College well-section as a vertical datum-line, on account of its proximity to the northern outcrop (which is not the case with the Goldsworthy section). But it does not stand alone, for the well-section at the Bagshot Orphan Asylum gives practically the same sequence, and affords strong evidence of the thinning northward of the above-named deposits. (Other instances cited by the author in 'Recent Contributions,' etc., corroborate the reading he has adopted.) The Goldsworthy section itself lends strong corroborative evidence as to the value of the College well-section. The evidence of attenuation in the direction of Bracknell the author reserves for the present. In his paper published in the February number of the Society's Journal for the current year, Mr. Monckton ignores the determinative value of stratigraphical alignment of the clays claimed as the basal clays of the Middle group with clays of the same character seen cropping out from below the 'green-earth series' at no great distance. This evidence of stratigraphical alignment must be allowed due weight when set against evidence derived from such lithological characters as the presence of pipe-clay, mica, and false-bedding. The author considers that the argument as to the fossil evidence is over-stated in the above-mentioned paper.

After criticizing some of the remarks in Mr. Monckton's paper, the author adds some notes on the sections at Farley Hill, Bearwood, and Wokingham.

3. "Notes on the Geology of the Nile Valley." By E. A. Johnson Pasha and H. Droop Richmond, Esq. (Communicated by Norman Tate, Esq., F.G.S.)

The rocks on either side of the Nile are chiefly Eocene (and Cretaceous?) from Cairo to Esneh; south of this is sandstone, which

the authors believe to be Carboniferous and to yield possible indications of coal, reaching to near Assouan, where it meets the granite and basalt of that region; a few miles south the sandstone begins again and continues to Wady Halfa, broken only by granite dykes.

The granite is intrusive into and alters the sandstone, whilst the latter reposes upon the basalt, and in some cases was deposited against upstanding basaltic masses. Unmistakable lavas occur near the Nile E. of Minieh and W. of Assiout.

A description of some remarkable faults is given, and various minerals are noticed as occurring in the sedimentary rocks and the bed of an ancient river.

CORRESPONDENCE.

PERMIAN IN DEVONSHIRE.

Sir,—After a careful perusal of the article on this subject in this month's Number of the present volume (pages 247–250) by Mr. W. A. E. Ussher, I am rather at a loss to see the actual drift of it. The author shows he is acquainted with several foreign geologists; and that he has seen the German Rothliegendes; he quotes opinions of one or two foreigners (notably Herr von Reinach) which corroborate my assignment in 1888 of the breccia-series of Devon to the age of the Rothliegendes (pp. 247, 248); and a little further down (p. 248) he puts forward a "probable correlation" of the Devon series of Teignmouth and Dawlish with rocks of the Nahe district, which agrees in all essential matters with the parallelism drawn in my papers (in the Q.J.G.S. of 1888 and 1892) between the Devon breccia-series and the Rothliegends of the Thüringen country, and of the country further east in Central Germany, particularly in the Gera district,—a parallelism based on previous first-hand knowledge of the German series. I commend these regions with that around the Hartz to any one who wishes to know the German Rothliegendes.

While, then, I entirely agree with Mr. Ussher that "no correlation framed on a partial or even intimate acquaintance with the *South Devon coast-section only* can be regarded as conclusive," I must ask you to allow me to remind readers of the Geol. Mag. of the two papers published by me in the year 1884, (1) "On the Dyas and Trias of Central Europe" (Q.J.G.S.), (2) "On the Permian-Trias Question" (Geol. Mag.); papers, of the existence of which Mr. Ussher could not have been ignorant at the time he penned the paragraph which opens with the above-quoted remark (pp. 248, 249), and of the value of which M. Jules Marcou expressed his strongest appreciation to me at the time.

The information given on p. 248 respecting the contemporaneous igneous rocks of the Nahe country adds little or nothing to our previous knowledge of them, since we read in Credner's 'Geologie' (6th edition, 1887), "The two lower groups [of the Rothliegendes of the Saar-Rheingebiet] are grouped by E. Weiss as Carboniferous Rothliegendes, described by von Dechen as Carboniferous rocks poor

in coal-seams. During their deposition very numerous eruptions of felsite-porphyry, melaphyre, palatinite, and porphyrite occurred, and formed dykes, intrusive layers, and tabular lava-flows (plattenfärmige Effusionsschichten) between the sedimentary rocks" (p. 516). Further details of the stratigraphy are also given (*loc. cit.*).

I am glad to find my contention in the paper which appeared last February (Q.J.G.S.) supported by the establishment by Professor Bücking of the lithological identity of some of those rocks with igneous rocks of the Devon Permian; although, as an argument based on the assumption of a strict temporal order of succession among igneous rocks, it has not much weighed with me. I must remind Mr. Ussher that those, who know how the basis of our classification of the *Midland* Permian and Trias was laid for us by the labours of Prof. Hull in former years, can hardly be expected to admit his statement on p. 249, that "the Midland sections owe their importance as a basis for correlation entirely to the merits of the assumed correctness of their classification with reference to the German types." The fact that the Devon and Midland areas were at the time disconnected does not affect the question, as a careful study of my papers, and that of Prof. Hull (1892), will make clear to any candid mind; papers based on observations by no means limited to the coast-section.

Wellington College, Berks. A. Irving.
13*th June*, 1892.

CONE-IN-CONE STRUCTURE.

Sir,—Mr. Young's statement that "The apices are invariably turned to the under or lower side of the structure while their bases are as invariably directed to the upper surface,"[1] is certainly not of universal application. In addition to examples instanced by Mr. Harker,[2] from the Lingula Flags and Lias shales, I may mention specimens of my own of concretions from the pencil slates in Swindale and from Carboniferous shale in Northumberland, which exhibit a similar radial arrangement of the cones. But the beds which afford the most striking refutation of Mr. Young's statement are the Coal seams, for it is in these beds that the structure is by far the most extensively developed portions of some seams several inches in thickness being made up of these cones. Examination of numerous specimens from the coal fields of Durham and South Wales show two systems of arrangement of the cones—one, where the cones have formed at right angles to certain laminae of deposition and on *both* sides of such laminae which are $\frac{1}{8}$–1 inch in thickness, so that the apices of the cones above point downwards, whilst those below point upwards, both sets of cones having evidently formed outwards from the same set of laminae. But the commonest disposition of the cones, especially in the Durham seams, is *parallel* to the bedding planes, and although the apices often run for some distance pointing in a constant direction, cases are of frequent occurrence where the bases of the cones start back, the apices being directed away from each other.

[1] Geol. Mag. for March, 1892, p. 138. [2] *Op. cit.* May, 1892, p. 240.

I do not see how Mr. Young's theory of the origin of Cone-in-cone structure by the upward escape of gases bringing up from below successive layers of plastic mud can possibly apply to the bulk of such concretions. E. J. Garwood.

FLEXIBLE SANDSTONE.

Sir,—I have read an interesting paper by Mr. G. W. Card, A.R.S.M., in the March number of the Geological Magazine "on the flexibility of rocks," and as there have of late been several allusions to this subject in the press. I venture to bring the following facts under your notice.

About eleven years ago a friend presented me with a piece of flexible sandstone which he had brought from India. I, in turn, gave the specimen to my friend and chief, the late Mr. C. S. Wilkinson, F.G.S., Government Geologist of N.S.W.

Mr. Wilkinson was greatly interested in the peculiarities of the stone, and after devoting some time to their investigation he informed me that he felt convinced that the flexibility was due to the presence of interstices between the grains of sand, and to the interlocking of the latter. He believed the interstices to be due to the shrinking of the cementing clay by loss of moisture, and in order to test his theory he immersed the specimen in water, with the result that after some time it became rigid. After again thoroughly drying the stone he found that its flexibility was completely restored.

Mr. Wilkinson was in the habit of showing this specimen and explaining the cause of its flexibility to visitors for some years before Mr. Oldham's paper on the Delhi sandstone was written, and there is no doubt in my mind that to him (Mr. Wilkinson) is due the credit of first recognising the cause of the flexibility of the Indian sandstone. Edward F. Pittman, A.R.S.M.

Geological Survey, N. S. Wales, Government Geologist.
Department of Mines, Sydney, 9*th May*, 1892.

OBITUARY.

STEPHEN AUSTIN.

Born 1804. Died 21st May, 1892.

By the death of Mr. Stephen Austin, in his 88th year, the Editor of this Journal has been deprived of an old and much valued friend, whose name must also now be familiar to all his Contributors as the printer of the Geological Magazine, since December 1865. The first two volumes (1864–65) were printed by Messrs. Spottiswoode & Co.; but the last twenty-seven volumes have been issued from the printing press of the well known firm of Messrs. S. Austin & Sons, Printers, Hertford, the excellence of whose work has largely contributed to maintain the reputation of this Journal during more than a quarter of a century that it has been in their hands.

This noted firm has been established in Hertford since 1768, having in that period passed through the hands of four generations of "Stephen Austins."

Mr. Austin's name will be best known to the world at large as one of the first and most celebrated printers of Oriental Literature.

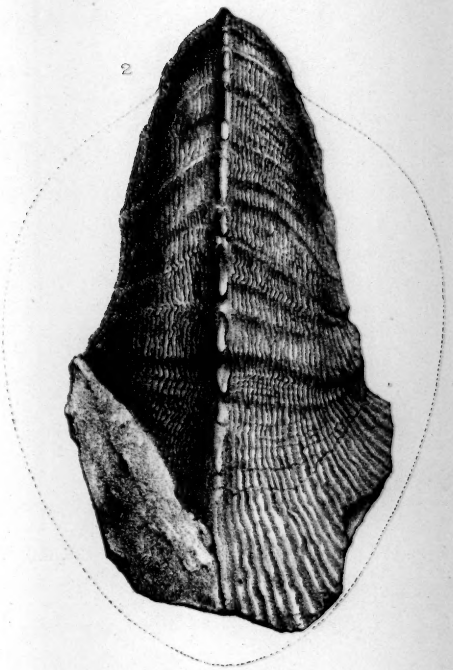
Mr. Austin and his father were the appointed printers and booksellers to the East India Company's College, the work of which while Haileybury was being built, was carried on at Hertford Castle. Mr. Stephen Austin retained that position until the Company was dissolved in 1858; and it was under the auspices of the authorities of that institution that he commenced the printing and publishing at Hertford of works in various Oriental languages. Up to that time great difficulty had been experienced in procuring the different Oriental books required by the students in their studies; those that were obtainable were only to be had at great cost, while the type used was so bad and the paper of such indifferent quality that the books were oftentimes almost illegible. It was somewhat of a revolution, therefore, when "The Hitopadesa" was printed with new Sanskrit type at Hertford in 1847, as at that date there were not more than one or two Oriental printers in England, and thenceforward during successive years a great number of books printed in Sanskrit, Bengálí, Arabic, Persian, Pushto, Hindustáni, Hindí, Hebrew, and other Eastern languages, as well as in Greek, Latin, and French, were issued from the Hertford Press of Stephen Austin, which in due time acquired a world-wide reputation for Oriental printing, and many of the finest specimens of Oriental typography now extant bear his name. The skill and taste displayed in these productions were acknowledged by the presentation to Mr. Austin of gold medals by her Majesty the Queen and the Empress of the French, by the award of medals of the first class at the International Exhibitions held in London and Paris, and by testimonials from many of the most eminent Oriental scholars of Europe and India; and in the year 1883 the Congrès International des Orientalistes presented their diploma to Mr. Austin for services rendered to Oriental literature.

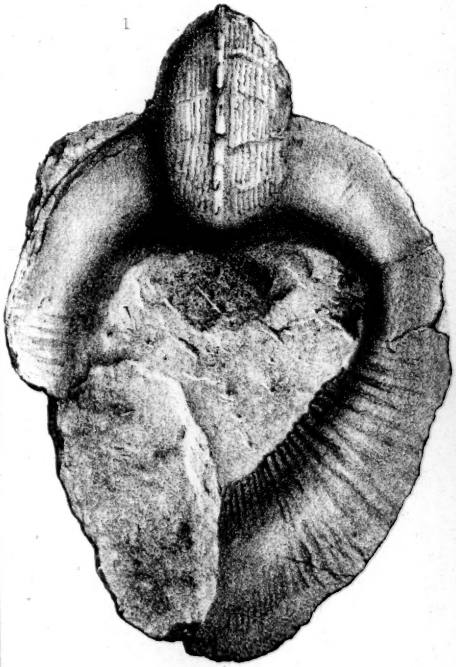
After the abolition of the East India Company's College at Haileybury, Mr. Austin was mainly instrumental in rescuing this historical place of learning from becoming an asylum or workhouse, and establishing the present successful Public School there; and in 1882 the Council of Haileybury College acknowledged Mr. Austin's valuable exertions by the presentation of a handsome service of plate.

In 1834 Mr. Austin established the "Reformer" Newspaper, now called the "Hertfordshire Mercury," which he has carried on successfully for more than fifty years.

It would be impossible to speak here of all the public offices Mr. Austin held during his long life in connection with his native town and county, or of the many marks of esteem and regard which he received. He leaves five sons and four daughters to cherish his memory, and who will share with many associates a tender regret for the loss to them of an honoured parent, and to us, of a dear and valued friend.

H. W.





Berjeau & Highley delet lith

West, Newman imp.

Trematonotus Britannicus, nov. sp.